SPECIAL FUNCTION

OF THE

SUDORIPAROUS

AND LYMPHATIC SYSTEMS

R. WILLIS

ON

THE SUDORIPAROUS

AND

LYMPHATIC GLANDULAR SYSTEMS;

THE VITAL NATURE OF THEIR FUNCTIONS IN PREPARING THE CONDITIONS NECESSARY TO VENOUS ABSORPTION, AND THE EFFECT OF THEIR DERANGEMENT IN THE PRODUCTION OF THE VARIOUS DISEASES ASCRIBED TO MALARIA.

BY

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Tout se reduit à recueillir des faits particuliers, et à rechercher des propositions générales qui en embrassent le plus grand nombre possible — CUVIER, *Histoire de Progrès des Sciences Naturelles.*

There is no appreciable intelligence in parts until some naturalist has ascertained their functions, and by the force of intellect divined their tributary relation to the whole. — R. W. MACKAY, *Idea of Christian Perfectibility.*

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TO

JOHN DAVY, ESQ., M.D., F.R.S.,

&c., &c.

DEAR SIR,

ALLOW me this opportunity of associating your name with these attempts of mine, by Inductive Reasoning from known facts, to resolve one or two of the obscure points in Animal Physics, that Science with which your name is so honourably connected. Let me farther crave permission to express the high sense I entertain of the value of your contributions to Physiological Science, and of your general merits as a Philosopher.

I am, dear Sir,

Yours, with much esteem,

ROBERT WILLIS.

PREFACE.

The work now given to my professional brethren comprises a connected and somewhat extended view of ideas and inductions that passed through my mind many years ago, and that were communicated at the time in fragmentary portions, and with less of development than I have always thought their importance required. I am no longer content to allow views which I believe to have been novel when they were enunciated, and which to the best of my knowledge and belief have not yet been seen either by physiologists or pathologists in the important light in which they still meet me, to lie buried in foot-notes and among the miscellaneous contributions of other writers in the pages of a periodical publication.

The view I have taken of the function of the sudoriparous glands, and of the lymphatic system of vessels, I am bold enough to maintain embodies a new principle in physiology, which explains the origination of that force whereby the transference of the fluids necessary to the nutrition and vital endowment of the living system from the efferent to the afferent channels of the circulation is effected, and throws a sufficiently satisfactory light on many facts in the domain both of health and disease that still awaited interpretation.

The facts upon which the inductions here presented in a connected series are based, have for the greater part—I might indeed say have all been long and familiarly known to physiologists; but this is commonly enough the case as regards the data which serve for the foundations of inductions that add most largely to the sum of human knowledge, and most essentially influence both the scientific and social progress of the world. The facts upon which Nicholas Copernicus founded his views of the Solar System were familiar enough to his predecessors and contemporaries. The facts upon which William Harvey raised his induction of the Circulation of the Blood were well known

to his teachers in anatomy and to the anatomists of his day. The belief that he who had had *the cow-disease* in his hands, was not likely to be attacked with small-pox, was current among the dairy men and women of Gloucestershire, and subject, doubtless, of occasional comment among the clergymen and medical practitioners of that county also; but it was only when Edward Jenner inferred that Vaccine might be designedly employed as a measure of defence against small-pox, that the world was presented with a stay against one of the most terrible scourges of humanity. Finally, and especially interesting to us as occurring in our own day and generation, the perturbations of the outermost known planet of our system were familiar to astronomers, and many and various, but still unsuccessful, attempts had been made to reduce these to order, in harmony with the laws of gravitation operating from *within* the orbit of the wayward star. But it was only when John Couch Adams and Urban I. I. Leverrier, assuming an *outer* cause of these disturbances, were enabled, in virtue of calculations based on inductive reasoning, to say: If such a cause exist, there or thereabouts will it be found, that the telescope of the keen-eyed observer directed to the point in the heavens that had been indicated, gave conscious birth to another world, and doubled the area of the vast realms of space in which our sun approves himself lord paramount. To descend from heaven to earth, and to compare small things with great,—in the same way, I say, physiologists, well enough aware of the importance of the cutaneous function to health and of the evil consequences that follow its implications; informed, moreover, by chemistry of the fact that almost the sole constituent of the sweat was pure water; conversant with the effects of radiation in reducing the temperature of the ground, and of objects placed immediately upon it, i. e., in causing *chills* as regards the human body, and fully certified through the experiments of the French physiologists of the true influences of hot-dry and hot-moist atmospheres, as well as of the fatal consequences that ensue from covering the surface of the animal body with impervious varnishes—

with all this mass of information before them, I say, physiologists and physicians have still gone on content or ill-content, as the case may be, with no explanation at all of the phenomena observed, with an explanation opposed to the legitimate interpretation of the facts adduced, or with an explanation of a purely hypothetical and undemonstrable nature. The very highest authorities in chemistry and animal physics—Berzelius and Johannes Müller—had, indeed, seen and expressed their dissatisfaction with this state of an important question, but they proposed no means of escape from the difficulties with which it was surrounded, and so left the door open for a new Induction upon the old accredited premisses. And this it is which the course of my studies and reflections led me to make now many years ago, and of which a long life devoted to the actual duties of my profession, and a constant study of the phenomena of health and disease, has never induced me to question the accuracy.

I have to request my readers' indulgence for the apparent controversial or negatively critical spirit that will here and there be apparent in my essays. I had in fact to clear the ground of what I apprehend to be false notions,—to get rid of the idea in especial that there exists in the animal economy a system for the purpose of dissipating heat,—a principle or force which underlies vital manifestation of every kind; which it is one essential duty of the digestive and respiratory systems to engender; and for the economizing of which, man and animals over so large a portion of the earth's surface are clothed in fur, wool, hair, and feathers, and where these fail—in circumstances where they would be found inconvenient and the temperature of the surrounding medium is low—have their bodies wrapped carefully round with a blanket of fat.

In the paper on the Lymphatic system there were few obstacles to be contended with. This system indeed appeared to be present in the economy without any function whatsoever; for the older ideas of its office had been abandoned, and none newer of a satisfactory nature had been substituted for these. I could therefore proceed at once to review and co-

ordinate the knowledge we possessed of the structure and distribution of the lymphatic system, and of the quality of the fluid it conveyed, in order to arrive at my inference in regard to its function, which I conclude to be of the same essential nature as that of the sudoriparous glands.

In my brief survey of some of the practical bearings of my physiological ideas, and particularly of their application to the subject of intermittent fever, and certain other diseases that so sorely try the European constitution in intertropical countries especially, I have necessarily again been forced upon negative criticism, and led to question the conclusions of some of the first authorities on these diseases. But in the uncertainty that is so apparent in the views and inferences of the distinguished observers quoted, I trust I show sufficient grounds for the liberty I have taken, and that I may say I have not ventured to pull down the old house without providing a shed at least for shelter.

Engaged as I have been for so many years in the laborious duties of a general practitioner of medicine, surgery, and obstetrics, and at some distance from libraries and readily accessible sources of information, I have to crave indulgence in case I should have overlooked what may have been added of late to the stock of our knowledge on the subjects that engage me. It may be indeed, for aught I know, that the facts I now adduce, and my inferences from them, have been presented in other shapes to the world since they first dawned upon my mind and engaged my serious thoughts. But lest it should be surmised that I appear in borrowed plumes, I beg to refer to my *notes* and *additions* to the "ELEMENTS OF PHYSIOLOGY" of Dr Rudolph Wagner, which I translated from the German, and published in 1842-43, and to the pages of the London *Medical Gazette* for 1843-44, for the enunciation of the ideas and inductions that are here presented in a co-related and connected series.

Barnes, Surrey, June, 1867.

ON

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The end for which the Cutaneous Exhalation exists has been avowed by the first authorities of the age in chemistry and animal physics not to be understood: "The quantity of solid matter," says Berzelius (Chimie, Trad. par Esslinger, tom. vii. p. 330), "which escapes from the body by the skin is extremely small, and all its constituents are besides encountered in the urine; the elimination of this matter cannot, therefore, bo viewed as the principal function of the skin. Water is, in fact, the chief matter thrown off by the skin; and in discussing the subject of animal heat, physiologists commonly agree in stating that the perspiration serves to regulate or depress the temperature of the body when it might be expected to rise under the influence of violent exercise, or any excessive heat of the surrounding atmosphere. But the intimate connection that subsists between the function of the skin and the bodily health proclaims it to have been instituted for yet another purpose, the nature of which is unknown to us."

This statement of the distinguished Swedish chemist is repeated, nearly in the same terms, by Professor Müller. He says briefly (Physiology, English Version, by Dr Baly, 2nd ed. p. 580), "The object of the cutaneous exhalation is not elucidated by its analysis; for the matters met with in it are

also constituents of the urine;" and it might have been added, of the organic fluids generally.

My views and inquiries lead me to conclude that, as the sweat is known to consist of from ninety-six to ninety-nine and a half per cent. of pure water, and only of from one-half to four per cent. of solids, THE PROPER OFFICE OF THE SUDORIPAROUS SYSTEM OF GLANDS IS TO ELIMINATE WATER ONLY, and that the saline and organic ingredients of its secretion are entirely adventitious. Water, however, unlike other excretions, is not only not deleterious to the economy, but is indispensable in every one of its operations; and is besides most obviously and imperatively required as the vehicle of the urea and salts of the urine. Wherefore, then, a system charged with the business of throwing off water? The answer usually given to this question is that upon which we have seen Berzelius remarking particularly; although, by the limitation in the middle of the paragraph quoted, and the concluding observations, with evident misgivings of its sufficiency: the object of the cutaneous exhalation, it is said, is to regulate, and, upon occasion, especially to reduce, the temperature of the body.

There are, however, many simply *à priori* considerations which should, I conceive, incline us to regard this particular duty as much more restricted than it is generally believed to be. In the uniformity, indeed, with which the more perfect animals exist in the world warmly clothed, and with which man clothes himself over four-fifths of the earth's surface—the end and influence of the clothing in every instance being to prevent the loss of heat consequent on exposure of the naked surface to the air—we have a sure indication that the animal body has but a limited capacity to engender heat, and therefore requires no special system for the purpose of keeping it cool. Nay, is there not even something like an absurdity involved in the idea of a particular system existing for the purpose of dissipating heat—heat, which is the indispensable agent, if it be not the immediate cause, of

every manifestation of vitality, and which it is one principal business of all the efforts of man over so large a portion of the earth's surface to economize or to maintain?—heat, which, as in our steam-engines so in our bodies, we see transformed into motion, and doubtless also expended in every act of the living organism, towards which it appears to stand in immediate co-relationship as cause? To say that the cutaneous exhalation exists for the purpose of cooling the body is surely to mistake a coincidence or accident for a purpose or specific end;—heat is lost, indeed, from the nude and even from the covered surface of the living body; but the loss here is only a necessary consequence of the body's contact with a medium lower in temperature than itself, and of the evaporation that ensues on exposure of a moist surface to the air.

The belief, still so general, in the regulating power, and, by an extension of the idea, in the refrigerating office of the cutaneous exhalation, is grounded on the experiments of our early English inquirers into the subject of animal heat, Drs Fordyce, Blagden, Solander, and Sir Joseph Banks.* To take the interpretation that is always given of these experiments—and indeed it is the implied, though not the expressed, inference of the experimenters themselves—we should conclude that a human being or an animal might remain for almost an unlimited length of time in a chamber heated to twice or more than twice the temperature of its body without detriment to its health, and without the acquisition of a single degree of heat. But the experiments of Fordyce and Blagden in the heated chamber do truly and in fact lead to no such conclusion as that which is constantly deduced from them; and I am utterly at a loss to conceive how physiologists should have gone on even to the present time interpreting these experiments as they have done.

Boerhaave† had stated as the result of his inquiries into the effects of heat on the living body, that an animal could not

* Philosophical Transactions, 1775.

† Elementa Chemiæ, vol. i. p. 275.

bear exposure to any excessive degree of heat without perishing. And this conclusion was quite legitimate on his part; for two dogs shut up by him and Fahrenheit in a stove-room heated to 146° Fahr. for an hour or more, both died. Dr Fordyce, moved in all probability by this statement of the great Leyden physician, which was opposed to popular belief, inasmuch as bakers' ovens at full heat were currently reported to have been frequently entered with impunity by the workmen—was the first to repeat his experiments, and very bravely selected his own person as the subject of his inquiries. Entering a room heated to excess by means of a red-hot cockle of iron, under various conditions of clothing, now stripped to his shirt, and again thickly clad in woollen garments, he found that he could bear exposure to a very high temperature *for a certain time*, without much or any inconvenience.

It would not appear to have been any part of the inquiry in this early stage to ascertain in what way the temperature of the body was affected by exposures of this kind, nor how long the exposure could be endured. It is noticed incidentally and by the way, however, that a thermometer applied under the tongue always indicated a rise in the temperature of the body above the standard — not much, indeed, yet above it by one or two degrees of Fahrenheit's scale; and we now know that the rise was not much greater only because the exposure was not more protracted. The excessive increase in the force and frequency of the pulse which Dr Fordyce observed might of itself have assured him as a physician that exposure to excessive heat could not be long continued with impunity. The dog, indeed, which he and Dr Blagden exposed in a wicker basket for about half an hour to an atmosphere heated to 220° F., and released when it had for some time been making desperate efforts to escape, and was doubtless very much distressed, was found to have had its temperature raised from 101° to 108° F. Had the creature been kept for fifteen or twenty minutes more in the stove, the accuracy of Boerhaave's conclusion would certainly

have been verified, and the views of the English inquirers thereby considerably modified. Farther, the subjects of every one of the experiments of Dr Dobson,* of Liverpool, the next inquirer into the effects of a heated atmosphere, were found in every instance to have had their temperature raised above the standard. This was most remarkably the case in the subject of his third experiment, a young and rather delicate lad, who after an exposure of no more than ten minutes in the stove heated to 220° F., had his internal temperature raised from 98° to 102° F.

These facts, which present themselves to me in so important a light, were made nothing of by the immediate observers, and have been, as it seems, all but persistently ignored by physiologists to the present time. This result is probably in no inconsiderable measure due to the series of experiments undertaken at a subsequent period by Drs Fordyce and Blagden, associated on this occasion with the Hon. Mr Phipps, Dr Solander, and Mr (Sir Joseph) Banks. Besides finding all their former inferences borne out, as they believed, by the new facts observed, they now arrived at the rather extraordinary conclusion: that the human body had not only a wonderful faculty of resisting great degrees of heat, but had farther an inherent power of engendering cold: a conclusion apparently come to on the strength of the fact that when the five individuals mentioned went together into a chamber, which we must presume not to have been of very large dimensions, heated to 198° F., the temperature fell rapidly to 110° F. Had five dead bodies instead of five living men been introduced into the chamber, the effect would undoubtedly have been the same in kind, though it may be not quite the same in degree.

The interesting and more recent researches of Messrs Delaroche and Berger,† which are frequently quoted as favouring

* Philosophical Transactions, 1775. Part 2.

† Expériences sur les effets qu'une forte chaleur produit sur l' économie animale.—Theses de Paris, 1806, No. 11. Journal de Physique, tom. 63, 71, 77.

the conclusions mistakenly drawn from the experiments of Fordyce and Blagden, are, without a single exception, directly opposed to them. Messrs Delaroche and Berger, operating with temperatures generally considerably lower than those employed by the English physiologists, observed that the heat of the animals which were the subjects of their experiments rose invariably and steadily; that their health became so seriously affected that few of them withstood exposure in the heated chamber for an hour without being reduced to extremity; and that when removed either in a dying state or at the moment of their death, their internal temperature was from 11·5° to 15·75° Fahr. higher than it had been on their entrance. Nor was the case different in regard to man; although, from the subjects of observation here not being shut up in the stove until they died, the rise of temperature noted was of course less remarkable. To discover that it was both signal enough and rapid enough, however, the 37th experiment, of which M. Delaroche was himself the subject, may be referred to. On entering the heated chamber, M. Delaroche's internal temperature was 29½ Reaumur; on quitting it, after no longer a stay than eight minutes, his internal temperature indicated 33½ Reaumur. In this very short space of time, therefore, the temperature of the internal parts of M. Delaroche's body had actually gained 4° of Reaumur, or 9° of Fahrenheit's scale.

In the memoir published several years subsequently to his inaugural dissertation,* M. Delaroche himself refers to the misinterpretation of his experiments, and the neglect of the true conclusions to which they lead. He says in express terms (l. c. p. 291). "J'ai vu constamment que la temperature des animaux exposés a une chaleur de plus de 35° ou 40° C. s'élevait d'une manière très marqué; que cette elevation de temperature allait jusque a 6° ou 7° C.; et je me suis

* Sur la cause de refroidissement qu'on observe chez les animaux exposés à une forte chaleur.—Journal de Physique, tom. 71, p. 289 et seq.

assuré que lorsque la chaleur extérieure est très considerable, cet accroissement de temperature n'a d'autre borne que la mort de l'animal, qui en est la suite necessaire . . . . La faculté de produire du froid," he continues, "est beaucoup plus restreinte qu'on ne le crois ordinairement."*

Discarding earlier inquiries, then, these experiments of Delaroche and Berger prove beyond all question, that the animal body has no power of resisting temperatures of the surrounding medium in any considerable degree higher than

* It is extraordinary to observe with what difficulty men, even of the most cultivated understanding and vast acquirement, disabuse themselves of false notions—the result of indoctrination—once these have taken possession of the mind. So distinguished a physiologist as Joannes Müller (Physiol. English Vers. 2nd ed. p. 85) pre-occupied apparently by the current ideas in regard to the heat-regulating powers of the cutaneous function, fails to see the experiments of the earlier English inquirers, as well as the later researches of the French, in their true bearings. He is particular in noting that Sir C. Blagden (it was Dr Fordyce) remained for eight minutes in a heat of 260° F. without damage ; but he makes little of the rise to the extent of 11°, 12°, and 15° F. observed by Delaroche and Berger in the subjects of their experiments, and nothing of the fact that exposure in the heated chamber continued beyond a certain limited time was invariably followed by death as the consequence. Dr Fordyce unquestionably bore the excessive heat to which he was exposed for eight minutes, in virtue of his bulk, of his clothing, and of the moisture with which the surface of his body was bedewed, upon the same principle, as regards the last factor, as the moistened hand may be plunged for an instant into a pot of molten metal without damage ; but the temperature of Dr Fordyce's body began to rise from the instant of his entering the stove ; and had he remained for some short time longer there, he would have discovered that he could no more do so with impunity than could the moistened hand be held plunged in the melted metal. Dr Carpenter, again, a man of vast erudition, the countryman of Fordyce and Dobson, of Black and Crawford, and with Delaroche and Berger experimenting, it may be said, under his eyes, is nevertheless found when speaking of 'the faculty possessed by the living system in man and the higher animals of keeping up its temperature to an elevated standard,' going on to say, 'and of *preventing it from being raised much beyond this by any degree of external heat.*' (Pr. of Phys. 5th ed. p. 412-13.) The fact, however, is the very opposite of this : *There is no limit to the rise in temperature of the animal body exposed to excessive degrees of heat.* Under such circumstances it gains 10°, 12°, 15° of Fahrenheit's scale more than the norm, when life ceases, and the body then goes on to roast or to burn like any other lifeless substance.

itself. Nor does this observation apply only to the excessive temperatures, such as 140°, 190°, and 220° F., which were used in the experiments of the English and French physiologists with the dry air stove; it applies equally to atmospheres of much inferior degrees of heat. Dr Davy, for instance, ascertained that the internal temperature of the companions of his voyage from England to Ceylon, rose gradually as warmer latitudes were gained; and, arrived at their destination in the island, that it was from 2·7° to 3·6° F. higher than it had been in England. He had already ascertained that the temperature of the common sheep was from 1° to 2°, and even 3° F. higher in summer than in winter; and Dr Edwards subsequently discovered that the temperature of the house-sparrow rose and fell with the season. In the month of February the mean temperature of the house-sparrow was found to be 105° F.; in April it was 108°, and in July 111° F. There can be no question of this law being quite general. Man, in especial, is fashioned in harmony with the varying circumstances in which he is placed as the native or visitant of every spot upon the surface of the earth; and if he have the wonderful attribute of preserving his temperature nearly at the same degree of the thermometer under the equator, and in the icy regions bordering upon the Poles, this is certainly in virtue of something else than the greater or less activity of the exhaling function of the skin.

The process by which heat is evolved in the animal body, as maintained by Black in the middle, and by Lavoisier towards the end of the last century, is now recognized not to differ from that by which it is generated in our stoves and furnaces. In these, with the freest access of air and abundance of fuel, there is a great evolution of heat; feebly blown, and scantily fed with combustible matter, there is little heat produced. In like manner the quantity of caloric evolved in the animal body is inseparably connected with the energy of the respiratory process, and the kind and quantity of the food that is consumed; and matters are so

adjusted by the great Creator of the universe that the degree in reference to man and the mammalia is uniformly hard upon 97° or 98° Fahr. In intertropical countries man lives, and thrives, naked or but thinly and partially clothed, on a relatively small amount of aliment principally derived from the vegetable kingdom; and here he breathes short, and consumes comparatively little oxygen; in the frozen regions of the extreme north and south, on the contrary, where he takes deep draughts of an air condensed by the cold according to its degree, and consumes a large amount of oxygen, and where he must have several pounds of animal food—the fatter the better—in the course of the twenty-four hours, he requires, in addition, to be thickly clad in wool and fur, in order to maintain his heat, and to live in health and vigour. Unprotected by clothing, at rest, and feeding mainly upon roots, grain, and fruit, the power to maintain a temperature of 97° or 98° F. appears to be in relation with an external mean temperature of about 80° F.; but shrouded in wool and fur, and well supplied with food abounding in hydro-carbonaceous matter, the standard is maintained, as we know, upon the shores of the Polar Seas.*

The temperature of the human body, however, is no constant quantity; it varies even in the state of health, as has been seen,—within narrow limits, indeed, yet it varies; in states of disease it presents much more decided fluctuations. Any rise above the standard, assuming this to be 97° or 98° F., will be effected, we must presume, from what has already been stated, in one of two ways: 1st, by an accelerated circulation of the blood, accompanied by a corresponding increase in the rate of the respiration; and, 2ndly, by the body being

* In connection with this point see Dr Crawford's classical work on Animal Heat, where many of the conclusions of more recent inquirers will be found anticipated, and the whole subject of thermopoesis in the animal body placed on its true footing. See also the Appendix to the Narrative of a Voyage to the North Pole, by Sir John Ross, and Dr Liebig's work, entitled, "Animal Chemistry in connection with Physiology."

so circumstanced that it loses little or none of its engendered caloric. And we accordingly find that the circulation and the respiration are alike augmented in force and frequency in those circumstances in which the temperature tends to rise, and does actually rise above the standard, as in acute febrile and inflammatory diseases, or when the native of a cold climate, with his capacious chest and voracious appetite, is suddenly transported to warmer latitudes. But there are very narrow limits to the rise of temperature under these circumstances: excitement in the animal body is ever speedily followed by languor and depression; if the current of the blood be driven more rapidly, the wave sent forth by each stroke of the heart is smaller; if the breathing be quickened, the volume of air inspired is less; and then the appetite for food failing, there is no longer a large supply of combustible material in the shape of aliment, and so the equilibrium is maintained. In short, it is quite certain that the human body, the heat of which is normally represented by 97° or 98° F., exposed to various temperatures, and under different circumstances of health and disease, does show variations within certain moderate yet readily appreciable limits.*

With regard to the second condition needful to a rise of temperature hinted at above, that, to wit, of the body losing none of its engendered caloric to the surrounding medium—this is a state of things that exists nowhere permanently on the earth's surface. The mean diurnal temperature of the hottest lands within the tropics does not exceed 82° or 84° F.—sixteen and eighteen degrees under that which is held to be the standard of the human body in temperate countries; by

* The fact of the temperature varying in different states of disease, especially, has very recently been made the subject of particular inquiry by pathologists, and there can be no doubt but that careful observation in this direction will yet lead to important conclusions in connection with the genesis, course, and conclusion of many diseases. Dr Wunderlich, whose papers I have cursorily perused in the Archiv für Heilkunde for 1866 and 1867, has, to the best of my knowledge, the merit of having lately led the way in this direction.

so much, consequently, must the body be warmed by its inherent powers, in order to maintain itself at its proper temperature. The heat of the air in intertropical regions, it is true, occasionally mounts to 100°, 106°, and even 110° F., but this is never generally, nor for any length of time; and it is certain that no degree of activity in the function of his skin then suffices to enable man to support for a succession of days such temperatures as these. In countries where the thermometer rises occasionally to 100° F. or more, the inhabitants have to betake themselves at such seasons to caves and cellars, where the temperature is much lower, and there remain supine; or they must sit exposed to air in motion, under Punkahs, &c. Forced to show themselves abroad and to use exertion of any kind, they speedily sink exhausted and die—their temperature having undoubtedly risen to a point that was incompatible with the continuance of life, precisely as happened to the animals which were the subjects of Delaroche and Berger's experiments in the heated chamber.

Under such circumstances, the effect which is generally regarded as the essential element in the function of the skin undoubtedly makes itself felt. But the action is then entirely physical, not vital: it is the mere consequence of the cooling that is inseparably connected with evaporation from a moist surface, and is seen taking place from the porous alcaraz or the caraffe in its moistened covering, in the same way as from the living skin. There is, however, no function of the animal body that only comes into play under peculiar external circumstances and over limited portions of the earth's surface; so that if a refrigerating faculty of the skin—the consequence of evaporation—might be found useful, as it would, within the tropics, and under other accidental circumstances, it is certain that whenever man feels the necessity of clothing himself, it would be useless. In temperate and cold climates a refrigerating faculty connected with the body in man, mammalia, and birds, is not only not required, but is even sedulously guarded against. Nevertheless, the action of the skin

is indispensable to life in every climate of the globe—at the arctic circle as within the tropics and under the equator.

And here a very important question immediately presents itself: Is the diminution or suppression of the perspiration, sensible and insensible, necessarily and invariably followed by a rise in the temperature of the body? It ought to be so if the views of the cutaneous function generally entertained are well founded.

In febrile and inflammatory diseases, it is familiarly known that there is an increase of temperature, and along with this, as is commonly said, a suppression of the cutaneous exhalation. That there is a diminution in the exhaling function of the skin during the hot periods of febrile diseases is certain; to assert that there is a total suppression, I believe to be an error, for a cold polished plate of metal brought close to the most burning skin will always become more or less dim. Does the same result follow when such a plate is approximated to the dry and shrivelled skin observed in the rigour that so generally precedes attacks of acute disease? and is the internal as well as the external temperature then depressed? In certain nervous hysterical or cataleptic attacks, accompanied by faintness and feebleness of the heart's action, the external temperature of this body certainly falls much below the standard. The whole surface of the trunk, as well as of the extremities, then feels icy cold to the healthy hand, and it is difficult to conceive that the internal temperature is not reduced also. I cannot however appeal to thermometric observation to show that such is the case. In the cold stage, as it is called, of paroxysmal fever, we have assurance on the best authority that the internal temperature rises and does not fall as the sensations of the patient would seem to indicate. Whether there be not an antecedent depression of temperature before the patient begins to complain, is to the best of my knowledge an open question; and we are yet without information on the state of the exhaling function of the skin at this period. In general anasarca, where the

cutaneous perspiration is more completely suppressed, perhaps, than it is in any other disease, there is always an icy state of surface, and, I believe, also a low grade of internal temperature; and in that curious affection of early infancy called sclerodermia, or skin-binding, in which I believe the proper function of the skin to be seriously implicated, one of the very remarkable features of the disease is the loss of vital warmth, which begins at an early period of the disorder, and in fatal cases—(and the great majority of such cases are fatal) — goes on continually increasing until life is extinct, when the internal as well as the external parts of the body, have been found 10°, 15°, 20°, and even 23° F. under the standard. Similar depressions of temperature, internally as well as externally, with other notable and formidable derangements of the vital functions, are well known to follow extensive destructions of the skin by scalds and burns, when its peculiar function is of course annihilated. Now the rise in the temperature generally which accompanies febrile diseases, when the exhaling function of the skin is certainly diminished, and the fall in the temperature under other circumstances, when the same process is implicated in fully as great a degree, assure us that the increase of temperature in the one case, and its fall in the other, depend on something which is not included in the commonly admitted theory of the cutaneous function. We have happily the evidence of other positive and most remarkable facts to prove that such is the case.

M. Foureault made what I cannot but regard as one of the most important and interesting physiological observations of the age, when he ascertained that an animal whose body was covered with an impervious glaze or varnish, suffered in every instance a notable derangement of its health; and, if the application of the glaze were tolerably complete, that the creature perished after the lapse of a certain time—a few hours only in some cases. Whether M. Foureault noted all the phenomena which precede and accompany the extinction of life

under such peculiar circumstances, does not appear from the notice of his Memoir, read before the Royal Academy of Sciences of Paris, which has been given to the public.* I presume that he had not; for the Messrs Becquerel and Breschet,† on repeating the experiments of M. Fourcault, are found remarking particularly on the fact that the suppression of the cutaneous exhalation was immediately followed not by a *rise*, but by a signal *fall*, in the temperature of the animal operated on, and this not only as regards the surface, but the deeper internal parts of the body also—a fall that went on increasing every instant from the beginning of the experiment till life was extinct, when the body was found to be but a few degrees Centigrade higher in temperature than that of the surrounding air. Thus, in their first experiment, Messrs Breschet and Becquerel observed that a rabbit, prepared in the manner of M. Fourcault, whose internal temperature at first was 38° Cent., had fallen in half an hour to 32° Cent., and in an hour to 24·5° Cent. In their second experiment,

* Comptes Rendus des Seances de l'Academie Royale des Sciences, &c., tom. vi. p. 369. In the notice here given, of M. Fourcault's first series of experiments, I observe that he refers the remarkable phenomena he witnessed to *impeded aëration or oxygenation of the skin*—'les vernis s'opposent a l'aëration et peutetre a l'oxigenation de la peau.' In the published account of his second series, M. Fourcault quits this ground, and now refers the implication of the vital functions that occurred to the retention within the body of *lactic acid*, a substance which we must therefore presume he thinks it the special business of the skin to eliminate from the system. But there is not a single fact in the whole range of animal physics that permits us to connect aëration or oxygenation of the blood in man, mammalia, and birds, with the skin, or indeed with any other than the pulmonic system. With regard to *lactic acid*, again, the substance to which the natural acid reaction of the sweat has been ascribed by most physiologists, this appears to be present in all the fluids of the body, and like other hydrocarbons is easily decompoundable and even available as a heat-producer when brought into contact with the air in the course of the pulmonic circulation. And, then, from 96 to 99·5 per cent. of pure water does seem a needless expenditure of power to free the system from mere traces of a substance which when not in excess appears to be perfectly innocuous, if not more positively useful. I say when *not in excess*, as I bring to mind the experiments of one of the most ingenious and gifted among the physicians of our age, Dr B. W. Richardson, he having discovered that lactic acid introduced artificially into the bodies of some of the lower animals excited all the symptoms of acute rheumatism as observed in the human subject.

† *Vide* Comptes Rendus, tom. xiii. p 791 *et seq.*

another rabbit, very carefully prepared, had fallen in one hour and a half, from 38° C. to 20° C.,—no more than three degrees above the temperature of the atmosphere, which, on the occasion, indicated 17° Cent. In one hour and a half more the animal had expired.

From all that precedes, it must, I think, be apparent that physiologists are either altogether without definite ideas as to the function of the sudoriparous system of glands, or that the ideas hitherto entertained by them on the subject, are erroneous. My own conviction is that the vital action of the skin has little or nothing to do with the maintenance of the standard temperature of the animal body. The real factors in this important process are the Digestive and Respiratory systems. Did all the other results of modern physiological inquiry not lead to this conclusion, as I hold that they do, the experiments of M. Chossat on the effects of starvation upon animals seem to place the fact beyond the sphere of question. Birds, rabbits, and guinea-pigs, kept without food, solid and liquid, till they died, were found to be but slightly affected in respect of their temperature during the first few days of abstinence—the natural diurnal variations being maintained to within 4 or 5 degrees F. As time ran on, however, and the effects of the alimentary privation were felt more severely, the decline in the heat was more decided; though it was only in the course of the last day of life that it became both rapid and excessive, the heat of the body as life was ebbing, and at the moment of its extinction, being actually found to have fallen by as many as 29·5° F. below the standard. So long as the respiratory system had any material to act upon, therefore, the heat was maintained to within a little of the proper height; when this was exhausted—consumed in the lungs—the heat fell, and vital manifestation ceased.*

* M. Chossat happily did not stop in his inquiries at the point indicated above. He went on to make an observation with which it strikes me that almost no other fact in physiology can be put in competition for its importance and significance. The subjects of his experiments—doves and pigeons in this instance—when reduced to extremity by want of food, when insensible and at the point of death, or when to all outward appearance they were actually dead, were very uniformly

Re-stating my Thesis: THAT THE OFFICE OF THE SUDORIPAROUS SYSTEM OF GLANDS IS TO ABSTRACT MERE WATER FROM THE PERIPHERAL CIRCULATION, I proceed to show the influence of this act upon the vital processes of the animal organism.

The ideas formerly entertained in regard to secretion and nutrition as effected by open-mouthed capillaries are no longer countenanced by physiologists: there is no such thing as an open-mouthed capillary vessel; capillary arteries uniformly end in capillary veins, and secretion and nutrition are accomplished by cellular developments and elective attractions between the substance of the several tissues and the appropriate matters held in solution by the general nutrient fluid—the blood. But the blood still contained within its conducting channels cannot be efficient as the vivifying fluid, any more than is the water supply to our towns of use to their inhabitants whilst in course of transmission by the canals and conduits that lead it from its source. Permeation of the conducting channels of the blood by its plasma, or proper nutrient element, is implied; and this is now known to take place abundantly throughout every point of the body: the spongy texture of the efferent canals in which the blood flows, particularly when put upon the stretch by the injecting force of the heart, permits the freest transudation to take place. But as in all continuous processes succession, or change, is implied, we have immediately to ask by what means the fluid thus incessantly transuded is made to find its way back into the circulation? That it should do so, if there was to be change, was obviously even as necessary as that it should escape. The answer always given by modern physiology to the

resuscitated by heat communicated to them artificially from without. In the warm atmosphere of a box adapted to the purpose they revived; their temperature rose, they regained their muscular powers, perched on the side of the box and even flew into the room, took the food that was offered them, pressed close to the sides of the heating apparatus, evidently enjoying the warmth it communicated, and the temperature being kept up for a length of time sufficient to enable the digestive system to get into play and supply the lungs with respiratory material, most of them recovered completely.

question now put is this :—the exuded fluids return into the veins by heterogeneous attraction or endosmosis. And this is true. But the condition on which endosmosis depends in the sense in which that remarkable phenomenon is usually understood, is the mediate contact of fluids having different qualities, or different densities. No means, however, have yet been assigned by physiologists for securing different degrees of density between the contents of the efferent and those of the afferent vessels,—for effecting a greater degree of density in the returning than in the outgoing current of the circulating fluid. Now this means I find in the action of the sudoriparous system, THE END AND IMPORT OF WHICH I REGARD AS SUBSERVIENT TO SECURING THE CONDITIONS NECESSARY TO THE RETURN INTO THE VENOUS CIRCULATION OF THE FLUIDS THAT HAVE BEEN SHED FROM THE ARTERIES FOR THE PURPOSES OF NUTRITION AND VITAL ENDOWMENT.

"Water," says Berzelius, "is the principal matter thrown off by the skin," a truth which is abundantly testified to in the analyses that have been made of the sweat by different chemists. Berzelius himself estimates the secretion to be composed of 99·5 per cent. of water and but 0·5 per cent. of solid matter,—matter which may be regarded as entirely adventitious, for it consists of the epithelial scales that are constantly being thrown off from the surface of the body, of the fatty product of the sebaceous follicles, and of simple saline ingredients. Later analyses do not differ essentially from this estimate of the great Swedish chemist. Anselmino gives from 5· to 12·5 in 1000 parts as the proportion of the solids to the fluid of the sweat. M. Favre assigns a still smaller relative proportion of solid to liquid matter, viz. 4·43 of solid to 1000 of liquid. Schotten raises the solids to 22·4 per 1000 parts, 12 of these being found to consist of the mere exuviæ of the skin.

We are safe in concluding, therefore, that the proportion of the solid matter to the fluid of the sweat is present in no higher a ratio than about one to the hundred parts : 99 per cent. is pure water, one per cent. is solid matter, and consists of com-

mon salt, epithelial scales, and the product of the sebaceous glands. It may not be unimportant here to observe upon the fact that there is nothing peculiar, or upon the most remote presumption, noxious in the solid matter of the sweat; so that it is vain to look in this direction for an explanation of the ill effects that follow implication of the function of the system by which it is secreted.

With this fact of the essentially watery constitution of the sweat before us, we perceive that the blood returning in the veins to the heart must necessarily be more dense than that which is leaving it, by the whole amount of watery fluid which has been abstracted and thrown off by the sudoriparous system of glands in the course of tho peripheral circulation.

That the quantity of water thus dissipated must vary greatly under different circumstances, follows as a necessary consequence when we come to understand the end and object for which it is purged away. Inappreciable at one time save by the nicest appliances, the sweat at another is so abundant as to trickle from the body in a stream, and form a pool about the feet. Seguin estimated the loss of fluid from the cutaneous and pulmonary systems conjointly, as taking place at the rate of about 18 grains per minute; of which quantity he held that 11 grains were due to the action of the skin, and 7 grains to that of the lungs. This estimate may probably be assumed as correct, the body being at rest, and the atmosphere in a state that would be characterized as temperate. But it is really impossible to speak of any particular amount of cutaneous exhalation as normal. Proceeding at all times, necessary to life under every variety of circumstance, the amount of the watery secretion of the skin varies from between one and two ounces in the course of an hour in a state of repose, and one, two, three, four, or even five pounds in the same interval of time under the influence of excessive exertion, in such an atmosphere as that of the stoke-hole of a steamship, a gas manufactory, or a glass house.*

* Vide The Philosophy of Health, by Southwood Smith, M.D.

That the blood undergoes inspissation to an extent corresponding in degree to the amount of its watery element abstracted, is a fact which direct analysis removes from the sphere of hypothesis to the realm of demonstration, since Dr Davy, in one of the many series of ingenious researches which he has instituted in the course of his useful life, ascertained that the specific gravity of arterial and venous blood, taken at the same moment from the same animal, differed materially in every instance, save one, where the two fluids were of the same density. Dr Davy found further, that there was a corresponding, but still more remarkable, difference in the specific gravity of the serum of the two kinds of blood. Taking the mean of the whole of Dr Davy's experiments, the specific gravity of venous blood turns out to be 1·053; that of arterial blood but 1·050. The specific gravity of the serum of venous blood, again, appears in the mean to be 1·026; that of the serum of arterial blood no more than 1·022.

The table following is taken from Dr Davy's Researches, Anatomical and Physiological, Vol. II. p. 24:—

Animal's age.			Art. bl.	Ven. bl.
Sheep	1	. . . . . .	1·050	1·056
,,	2	6 yrs.	1·057	1·058
,,	3	mo.	1·049	1·051
,,	4	. . . . . .	1·047	1·050
,,	5	3 yrs.	1·047	1·051
Lamb	1	11 weeks	1·052	1·055
,,	2	,,	1·046	1·057
,,	3	,,	1·054	1·054
,,	4	,,	1·050	1·053
,,	5	,,	1·047	1·050
Ox	.	. . . . . .	1·058	1·061
Calf	.	. . . . . .	1·040	1·046
Dog	.	. . . . . .	1·048	1·053
Mean	.	. . . . . .	1·050	1·053

Dr Herman Nasse, in his work on the blood,* informs us that his researches agree, in almost every particular, with those of Dr Davy. In every one of his trials he found the venous blood of somewhat higher density than the arterial blood, the difference being generally about ·03 in favour of the venous fluid; and on no occasion did he find the serum of the venous blood of lower specific gravity than that of the arterial blood; on the contrary, it was generally appreciably heavier, the amount in its favour falling between ·05 and ·04 in 1000.

Hering† also ascertained that the venous blood of the ox, sheep, and horse contained less water than the arterial blood. The mean of all his trials gives about 825·6 for the quantity of water contained in 1000 parts of arterial blood, and 812·0 for the quantity contained in 1000 parts of venous blood.

Dr F. Simon‡ of Berlin, has more recently confirmed these interesting results obtained by Davy, Nasse, Hering, and others, not only in regard to arterial and venous blood at large, but has extended the sphere of his inquiries to the arterial and venous, or out-going and in-coming, currents of several particular organs or systems. The sum of his researches is this, that the arterial or out-going current was constantly found to contain a larger quantity of water, i. e., to be of less density than the venous or returning current—the difference, as might have been predicated, being greatest in reference to the blood of those organs where there was the most abundant separation of watery fluid.

In repeating these experiments upon the blood of the sheep, I have myself very regularly found the serum of venous blood of somewhat greater density than that of arterial blood; and even when, through imperfection of apparatus, the difference

* Das Blut in mehrfacher Beziehung. Bonn, 1836.

† Physiologie für Thierärzte. Stuttgard, 1832.

‡ Handb. der medicinischen angewandten Chemie. Berlin, 1841-1842.

could only be seen as extremely slight, or was inappreciable, on putting quantities of the two kinds of serum into an apparatus for showing endosmosis, I have still seen a current of considerable power established from the arterial to the venous side. THIS IS THE PROCESS, IN FACT, WHICH, AS CARRIED ON BETWEEN THE ARTERIES AND VEINS OF A LIVING CREATURE, CONSTITUTES VENOUS ABSORPTION, THE CONDITIONS NECESSARY TO WHICH, VIZ., THE HIGHER DENSITY OF THE BLOOD IN THE VEINS OR RETURNING VESSELS THAN IN THE ARTERIES OR EFFERENT VESSELS IN ALL THE PERIPHERAL PARTS OF THE BODY BEING MAINLY DUE TO THE ACTION OF THE SUDORIPAROUS GLANDS.* In this way we perceive at once how the elimination of so much simple water from the surface of the body becomes so indispensable to health and life as it unquestionably is.

In connection with the office here assigned to the sudoriparous glands, it will not appear out of place if I pause to speak of the extent of apparatus dedicated to this, as I maintain among the number of the vitally important processes carried on in the animal economy. Erasmus Wilson,† whose labours in connection with diseases of the skin, are so well known and so justly appreciated, has ascertained that there are no fewer than 3528 sweat glands contained in each square inch of the palm of the hand; and as each of these tubular glands when straightened from the convoluted and spiral form in which it exists, measures about a quarter of an inch, it follows that in every square inch of the palm there is stored a length of secreting tube equal to 882 inches or 73½ feet! The number of sweat glands Mr Wilson finds to be sometimes greater, more frequently less, than in the palm; but assuming 2800 as the mean over every square inch of surface, and the number of square inches in the body of a man of average height at 2500,

* I say *mainly* here, for in my next essay, on the lymphatic system, I shall show its function as complimentary to and of the same essential nature as that of the skin.

† Erasmus Wilson, on the Management of the Skin, 3rd ed., p. 37.

the whole number of sweat glands contained in the skin is found to amount to seven millions at least; and the length of these tubular glands, were they deployed, would be 1,570,000 inches, or as nearly as possible 28 miles!

It is farther not unimportant to observe that the skin, in health, is especially aroused to activity under circumstances that increase the force and frequency of the heart's contractions: we perspire more copiously and freely as we use more violent exertion, and as the air around us is hotter and drier. Wherefore is this? We do not observe that any of the other glands or glandular systems are excited to peculiar action under the same circumstances: the salivary and mucous glands of the mouth and throat do not then secrete more saliva than usual; on the contrary they pour out less, so that the mouth becomes clammy and dry. The kidneys do not elaborate more urine than wont; they rather eliminate less, &c., &c. The usual explanation given of the diminished activity of the salivary glands and kidneys is anything but satisfactory; the blood, it is said, being circulated in larger quantity to the capillaries of the skin, is therefore sent in less measure to the internal organs. But it is obvious that, as the heart injects a common trunk, which feeds the vessels of every part of the body, if there be more blood passing through the capillary net-work of the skin during active exertion, there must, in like manner be more passing through that of the kidney and of every other element of the body. Wherefore, then, the peculiar activity of the glandular apparatus of the skin under the circumstances indicated? First, because there is, with the accompanying augmented vigour of respiration and circulation, a more rapid consumption of the carbonaceous elements of the body, and a consequent greater evolution of the heat that is required for conversion into motion and vital action of every kind—innervation especially;* second, because

* Vide, in connection with this interesting topic, the admirable essay of Mr Grove: "The Correlation and Continuity of Physical Forces," 5th Ed. London, 1867.

the function of the glandular apparatus of the skin is essential to secure the return into the venous system of the plasma which, under the influence of the extraordinary injecting force of the heart, and to meet the increased demand for vital endowment, is transuding the coats of the capillary arteries in unusual quantity : the returning stream must be inspissated in an unusual degree in order to have the endosmotic current from the plasma shed into the areolæ and interstices of the component molecules of the body back into the veins, of power sufficient to keep the system drained of the fluid that is tending to overwhelm it.

We observe, still farther, that the sudoriparous glands are largest and most active in the precise situations where their function is most required, and also where it is calculated to prove of greatest avail, viz., the hands and feet. Insensible in every other part of the body, the perspiration is commonly enough sensible in the palms and soles ; it never fails to become so under very slight exertion of any kind. The sudoriparous system of the skin has this peculiarity also, which we observe in none other (unless, indeed, the lymphatic system be regarded as possessing the essential constitution of an universally distributed gland, as I shall by-and-by show that it must), viz., that *it is not concentrated but diffused :* it is spread over the entire surface of the body ; an arrangement which leads us to infer that its function is also needful over the entire periphery ; for we very certainly find glandular organs in the neighbourhood of parts where their office or the products of their activity are required.

The value of the power engendered by the difference of density between the out-going and in-coming currents of the circulating fluid, in explaining morbid phenomena of several kinds, will be at once appreciated by the pathologist. It was, indeed, whilst engaged in studying the pathology of scarlatina, and of those diseases that are accompanied by effusion, and after having satisfied myself of the insufficiency of the generally received theories of the phenomena which attend

them, that I was led to look farther, and to discover a powerful and, as I believe, a hitherto neglected cause of these phenomena in whatever interferes with the accomplishment of the conditions that are necessary to secure the normal difference of density between the arterial and venous portions of the circulating fluid. Whatever interferes with the blood in the arterial capillary vessels losing a portion of its water—the water, as regards the entire surface of the body, being thrown off by the sweat glands of the skin—immediately compromises vital function; and, the cause of interference continuing to operate, and these glands remaining implicated in their function, disease and death inevitably follow.

Thus far my attention has been directed exclusively to that which I regard as the proper function of the sudoriparous system of glands. But there is yet another phenomenon which is occasionally and accidentally presented by the animal body in the higher grades of life, and which appears to be constant and necessary in several of the lower, to which I have not adverted. This is a power of absorption or imbibition possessed by the skin; an act wherein, if it occur at all, the pores by which the sudoriparous glands end on the surface, must be presumed to play some, if not a necessary, part. Such a faculty as this, even in the highest manifestation of life presented to us, that of man, might almost seem a necessity in the nature of things: the skin is unquestionably one in the series of membranes through which, or by means of which, heterogeneous attraction is fitted to take place; and the conditions necessary to the manifestation of this natural process being present, it may be said to ensue as a matter of course. The blood of a man and the fluids that bathe the areolæ of his bodily tissues, when he is athirst through privation, or when he has lost several pounds' weight of water by exhalation from the skin and lungs under exertion, are in a very different state as regards density from what they are shortly after a full meal of solid and liquid aliment. In the former state, the blood must approach the

treacly consistency which is observed in certain morbid conditions of the body—diabetes mellitus, and Asiatic cholera—to cite an extreme, and the extremest, case known; and is then in the fittest possible condition to imbibe such a fluid as water, possessed of a density so much inferior to its own. It is matter of notoriety that mariners shipwrecked, or confined to the caps and shrouds of water-logged vessels, endure privation of water for very long periods of time when their clothing is, of course, kept constantly saturated by the spray of the sea. Some few months ago we had the well-authenticated history of the master of a water-logged ship who survived, for 28 days' privation of all solid food, with such scanty and distant supplies of water only as he could collect in the palm of his hand, when rain fell so heavily as to trickle down the mast. But he was constantly exposed to the spray of the sea, and his clothing, which was happily abundant, was almost uniformly in a state of saturation. The rest of the crew, under precisely the same circumstances as the master, but scantily clothed, dropped off one after another at different intervals, according to their capacity, as it would seem, to resist the benumbing effects of cold and privation.

Without dwelling upon such extreme cases as the above, it appears pretty well authenticated that persons athirst through exertion and somewhat protracted abstinence from liquids, find their drought abated by immersion in a warm bath. Dr Madden* found that, despite the continued loss by pulmonary and cutaneous transudation, the body generally gained in weight during immersion in a warm bath—the increase, in some instances, being as much as five drachms and a half in the course of half an hour, and as the loss of weight during the previous half-hour had been six drachms and a half, supposing the loss to have proceeded at the same rate during the immersion in the bath—which is by no means certain—the gain by imbibition through the skin in the course of the half-

* Experimental Inquiry into the Physiology of Cutaneous Absorption.

hour must have amounted to more than an ounce and a half. Dr Southwood Smith mentions the case of a man, the subject of one of his experiments, who having lost 3 lbs. weight by perspiration and pulmonary exhalation during more than an hour's hard work in a very hot atmosphere, regained 8 ounces by immersion in a warm bath at 95° F. for half an hour.

This subject is very fully discussed by Dr Carpenter,* to whom I have to acknowledge my immediate indebtedness for acquaintance with many of the facts here adduced. Various other illustrative instances referred to by Dr Carpenter are, in my opinion, either incredible in themselves, or without that character of accuracy which is now held imperative as giving scientific value to any extraordinary statement. In Dr Currie's case of the patient affected with dysphagia, for instance, whose thirst was abated, and whose life was probably prolonged, by the exhibition of enemata and the use of the warm bath, it is impossible, as Dr Carpenter justly observes, to discriminate between the influence of the enemata and that of the bath. In the case of Dr Lining, again, who believed that, upon one occasion, he had gained $8\frac{1}{2}$ ounces in two hours [Qy from a moist atmosphere or a warm bath], such being the difference in the weight of his person at the beginning and the end of the interval mentioned, and allowance made for the ingesta and the egesta during the same time. But the Doctor appears to have been regaling himself during the trial with both solid and liquid aliment, and though he took note of the fluids he took none of the water contained in the solids he consumed. As to Dr Watson's case of the lad at Newmarket, who by starvation and sweating had been brought down to the proper weight for riding a match, and who, between the hours of 9 a.m., when he was of the weight required, and 10 a.m., when he was found to have gained as much as 30 oz., "though he had only drunk half a glass of wine in the interval," I should beg to be allowed, as is done in parliamentary

* Principles of Human Physiology, 5th ed., pp. 112, 113.

proceedings, between the words "only" and "drunk" to add these words "been known to have." The poor boy—mad with thirst and the cruel treatment he had undergone—had doubtless found opportunity in the relaxation of discipline that is so apt to occur on the immediate eve of an anticipated event to get at a meal of meat and a pot of ale, which would just account for the 30 ounces of gain. I am also compelled to criticise Dr Carpenter's own case, as reported to him by Sir George Hill, the particulars of which are so nearly identical with those of the Newmarket lad, that we shall not feel ourselves guilty of any excessive amount of scepticism if we refer all that is miraculous about it to the same cause. Sir G. Hill's jockey, brought down by the usual processes to the proper weight, being greatly distressed by thirst on the morning of the race, "took one cup of tea," and shortly afterwards, marvellous to relate, "he was found to have increased six pounds, so that he was incapacitated for riding." Dr Carpenter, with what does certainly look like an easiness of faith which we should not expect in a man of science, finds nothing suspicious in these statements, but goes on to remark, "that nearly the whole of the increase in these cases must be attributed to absorption from the vapour of the atmosphere, probably, however, rather through the lungs than through the skin." "Half a glass of sherry," and "one cup of tea," good things in themselves and highly restorative in their season, would seem capable upon occasion of producing effects hitherto undreamt of in our philosophy. Why the half glass of wine and the cup of tea should have been necessary to set the skin and the lungs off in the business of absorption does not appear.

Müller is much more brief and sceptical on the subject of cutaneous absorption than Dr Carpenter. He says,* with truly philosophical caution, "It has long been a contested question whether the skin, covered with its epidermis, has the power of absorbing water; and it is a point difficult to determine,

* Physiology, Eng., Vers. 2nd ed., p. 269.

because the skin loses water by evaporation." He repudiates the conclusions of all the experimenters, Falconer, Alexander, Seguin, and Currie, whom he quotes on the subject. His translator, Dr Baly, however, cites, with something like approval, Dr Madden's experiments, and those of M. Berthold, which seem to favour the theory of cutaneous absorption. For my own part, I still see that the whole of their conclusions are open to criticism; the cuticle, as Müller observes, is itself hygroscopic, and there is no attempt made in any of the experiments to distinguish between mere physical imbibition by the excrementitious cuticle, and penetration of the living tissues and blood-vessels by water in contact with the surface of the body. Who that has had his hands for some time plunged in hot water, particularly if it contain any alkaline substance in solution, can doubt of the very considerable hygrometric power of the cuticle? It seems to me possible that the whole of the increase in weight observed by Dr Madden in his trials, and even in the one of Dr S. Smith which I have quoted particularly, might be fairly referred to this cause alone.

Whilst absorption through the skin into the circulation in man and the higher mammalia, then, remains an open question, there can be no doubt of such a process existing, as both normal and necessary, among some of the lower forms of animal life. Frogs, toads, and lizards, for instance, after having lost a very considerable part of their weight by being kept for a time without food in a dry atmosphere, and when they look shrivelled and ill, recover both weight and plumpness very rapidly when their bodies are but partially immersed in water. Nay, an atmosphere saturated with moisture is said to suffice for the restoration of the loss which these creatures undergo under the circumstances indicated. The fact of such a power, undoubtedly possessed by the Batrachians especially, has probably led to the more persistent assumption of an absorbing faculty for the skin of man than any truly scientific data extant would seem to warrant.

ON THE FUNCTION

OF THE

LYMPHATIC SYSTEM OF VESSELS,

WITH A NOTE

ON THE SPLEEN AS ONE OF ITS ELEMENTS.

All the advances lately made in physiology, instead of rendering more obvious the end for which the vessels entitled *lymphatics or absorbents* exist, seem but to have made their presence in the economy a greater anomaly, their function a greater enigma. After having been left for half a century or more in exclusive possession of the office of *absorbing*, M. Magendie especially has held himself authorized to conclude from his experiments, that the lymphatics have nothing to do with absorption; this act, according to him, being performed exclusively by the veins. It is needless to say that these conclusions of the distinguished physiologist named have now been generally adopted. But while M. Magendie thus deprived the lymphatics of their ancient office, he did not venture to assign to them any new function, and has consequently left them to be considered as a superfluity—a something that might have been dispensed with in the nicely ordered mechanism of the animal body; where, however, there is very certainly nothing superfluous—no organ without its definite

function, as there is no function without its appropriate instrument.

Professor Rudolph Wagner can see no reason for the existence of the lymphatic system, "Neither anatomical nor physiological considerations," he remarks,* "render any satisfactory account of the import and office of the lymphatics;" and that accomplished anatomist, Mr Lane,† whilst he shows very satisfactorily that neither in structure nor distribution can the lymphatic system be connected with the functions assigned to it by the school of the Hunters and Monros, has himself no better suggestion to make in regard to its office than this: to "select and prepare nutritious materials for the purposes of sanguification."‡ The field, therefore, seems as open to inquiry, in regard to the end for which this system of vessels

* Physiology, English version, by Dr Willis, p. 441.

† Cyclop. of Anat. and Physiol., Art. Lymphatic System.

‡ This assumption of the lymphatic system as the laboratory of the distinguishing elements of the blood (Lane), or otherwise, as the organ of 'nutritive absorption' (Carpenter), is obviously opposed to the views of Hunter and his followers, who held that the business of the lymphatics was to take up effete matters, the products of nutrition completed, to the end that these might be eliminated from the body. There are, however, as many and as grave objections to the modern idea as to the one of older date.

That both lymph and blood-corpuscles are generated in greater or smaller number within the lymphatic vessels is probably true, and is perhaps no more than might have been expected *à priori*: the lymphatics are an element in connection with the general circulating system; and that they contain no special effete or deleterious matter requiring elimination from the body, is made obvious from the fact that their contents are poured into the general circulation near the heart, and that the lacteal portion of the system is the channel by which the whole nutriment of the body is received. There is no reason for looking beyond the blood itself as the source of its distinguishing cell-elements—elements which, as in every other instance, are in a ceaseless state of genesis and decay; the cell-elements here being immediately engendered from the nutrient materials supplied by the lacteals, chemically and vitally altered by exposure to the air of the atmosphere in the lungs. Nor need the state of freedom in which the blood-corpuscles exist cause any difficulty; they are no single and solitary instance of cells existing in such a condition and endowed with powers of evolution: the female ovum and male spermatozoon are other instances of cells set free from all connection with the organs in which they are produced, yet possessed of the most remarkable vitalizing powers.

exists, as it is in regard to the office of the spleen, and of the thymus and thyroid glands.

In my essay "On the Function of the Sudoriparous System of Glands," I show the influence which the dissipation by its agency of a certain amount of pure water exerts upon the current of blood returning from all the peripheral parts of the body to the heart, and insist upon this as the means by which venous endosmosis or absorption is mainly secured.

In perusing what has lately been written of highest authority on the subject of absorption, it will be seen that though the necessity for a difference between the fluid that is ministering to the nutrition, or feeding the secreting faculty of an organ, and the fluid that is returning from this organ having done its office, may very constantly have been felt, the means by which this needful difference was effected, to say nothing of its nature, has not hitherto been explained. Imbibition, or heterogeneous attraction, to which we now refer all absorption, in entire contravention of the views entertained by physiologists of the last generation, is always understood as taking place through membranes interposed between fluids or matters in a state other than that of homogeneousness or equiponderance. Were the several elements, both fluid and solid, of the tissues which compose the animal body all in the same state, chemically and physically, there would be no interchange of matter between them :—the walls of the arteries might exude, indeed, but those of the veins would not imbibe. "If the same portion of blood were constantly exposed to the same part of the tissues," says Müller,* "imbibition after a time would necessarily cease. The motion of the blood must be so far favourable to imbibition, therefore, as it removes what has already been absorbed, and thus renders constant the cause of endosmosis." The necessity for a difference to secure osmosis between the out-going and in-coming

* Physiology, English version, by Dr Baly, 2nd ed., p. 267.

currents of blood, is distinctly expressed in this passage; but neither here, nor in any other part of Müller's work, is there a reason assigned for any difference that may perchance exist between the one and the other. To say that the arteries exude and the veins imbibe in the course of the circulation, is to state an admitted fact, but not to make known the means by which the former are disposed to exude and the latter fitted to imbibe. Did not the blood, between its outward and inward course, lose something by which its quality was altered, by which its density especially was augmented, there would be no endosmosis into the returning channels, in the sense in which the word is understood in physiology, viz., penetration with increase in the bulk of the body or liquid penetrated.

I have, as I conceive, shown that the sudoriparous glands form one element in a great system existing for the especial purpose of abstracting water from the blood, and thus preparing the conditions essential to the return into the venous circulation of the fluids transuded by the arteries for the purposes of nutrition and vital endowment—I say one element in the system having this important office entrusted to it; for the influence of the sudoriparous glands could not be expected to extend to any great depth below the surface. The subcutaneous veins transmitting a fluid more dense than that of the arteries which feed the superficial parts of the body by the whole amount of the sweat, sensible and insensible, which has been thrown off, will effectually drain the tissues through which they pass; but they would exert little or no influence upon deeper parts; these would still be liable either to be overwhelmed by the flood of plasma poured upon them by the nutrient arteries; or otherwise, there being no attraction exerted between the tissues of these parts and the contents of the capillary arteries, exudation as well as imbibition would cease. It appears to me that nature has come to the aid of these deeper tissues by means of the vessels entitled LYMPHATICS OF ABSORBENTS, ONE ESSENTIAL FUNCTION OF WHICH I BELIEVE TO BE THE ABSTRACTION OF A CERTAIN QUANTITY OF

THE WATERY ELEMENT OF THE BLOOD, FOR THE SPECIFIC END OF RENDERING THE RETURNING STREAM IN THE DEEPER SEATED PARTS OF THE BODY OF GREATER DENSITY THAN THE OUT-GOING STREAM.

The very first question that now presents itself is this: what is the nature of the fluid which the lymphatic vessels transport? Is it watery and of less density, or is it thick and of greater density, than the blood, plasma, or liquor sanguinis? If it were more dense than the liquor sanguinis, it could have no influence in augmenting the density of the blood which is returning in the veins to the heart; if less dense, on the contrary, in proportion to the excess of its water over that of the blood, will be its influence in causing condensation of this fluid in the veins, and in fitting them to drain the tissues through which they pass. Now the samples of human lymph, as well as of the lymph of healthy quadrupeds, which have been analyzed, have all been found of density greatly inferior to that of the blood or liquor sanguinis. In the lymph examined by Marchand and Colberg, which was obtained from an open lymphatic vessel upon the instep, the quantity of water amounted to 96·92 in 100 parts. In that analyzed by Bergman, which was obtained from another subject under similar circumstances, the quantity of water was 96·10 in 100 parts. And in that examined by Dr G. O. Rees, which was obtained from the abdominal lymphatics of a healthy donkey, the quantity of water was 96·53 in the 100 parts. These results, obtained by different chemists of acknowledged accuracy, at different times, agree very remarkably, and may be taken without limitation as correct. But the quantity of water which enters into the composition of the blood of man appears, from the multiplied analyses of M. Lecanu, to amount to no more than from 77·8 to 82·7 in the 100 parts. The lymph is therefore a much more attenuated fluid than the blood, and being so, the blood will be inspissated, and made apt to imbibe in the same ratio in which water, represented by the watery lymph of the lymphatic vessels, is abstracted from its mass.

The difficulty that presents itself to the mind at this stage of my inquiry, is to conceive the means by which the lymph is separated from the blood. It cannot be by a merely mechanical or physical process akin to or identical with that implied in osmosis. Magendie and his scholars refused all absorbing power to the lymphatics, as we have seen; and if the expression be used to signify physical imbibition to a marked extent, as it is carried on by the veins, I believe that they did so with perfect propriety. The veins are, in fact, the *only absorbents* in the strict sense of that word; i.e., they are the only structures whose office, in addition to that which they have as channels of the returning current of blood, is connected with or dependent upon their capacity to imbibe. Even the lacteals, as I shall have occasion to show, form no exception to this rule. All the tissues of the body imbibe, it is true; but this they do very slowly in every instance, save in that of the veins; and except in reference to these vessels, and, in some measure perhaps to the intestinal villi, function seems in no case to be connected with the mere physical capacity to be readily penetrated by fluids. Function is, in fact, very often directly opposed to osmosis, as witness the separation from the relatively dense blood of the watery saliva, pancreatic juice, sweat, and urine. The admitted laws of osmosis declare that such fluids as these ought not to quit—cannot quit the blood. There can be no question, however, of every tissue and organ in the body being endowed with an especial elective and divellent power, connected doubtless with its development and growth, by which it selects from the plasma with which it is bathed, the peculiar principles required for its formation and nutrition: bone takes gelatine and phosphate of lime; muscle takes fibrine; the brain and nerves take albuminous, fatty, and phosphoric matters, &c. And then the glandular organs have very peculiar, if they have not, perchance, certain additional, formative powers, by which, whilst they are nourished, they at the same time separate from the blood, or prepare and pour into appropriate

channels, the various fluids which are spoken of as their secretions.

In the same way precisely would it seem that the lymphatics and lacteal vessels perform their functions. THESE VESSELS MAY, IN FACT, BE VIEWED AS THE ESSENTIAL ELEMENTS OF A FILIFORM AND ALL BUT UNIVERSALLY DISTRIBUTED GLAND. Their walls and sacculated commencements—for they do not begin like veins in microscopic capillaries—have been shown by the latest and best observers* to be composed of an aggregation of granular matter, of nuclei, and of cells in various stages of development, precisely similar, as I conceive, to the ultimate elements of every secreting organ known. Here, as elsewhere, nuclei, in a ceaseless state of reproduction, attract a quantity of granular matter about them, and then they become surrounded with a delicate envelope filled with fluid—they have grown into cells; and these having attained maturity, and being then in contact with or produced upon the internal surface of the lymphatic vessels in whose walls they have grown, they give way and shed their contents into their cavities. The secreted fluid in reference to the lymphatic system is the lymph,—a fluid having on an average, as has been seen, about $96\frac{1}{2}$ per cent. of water and $3\frac{1}{2}$ per cent. of solid matter in its constitution, this solid matter being identical in chemical composition with that of the liquor sanguinis.†

The lymphatics then, or, to speak more accurately, the cells of which their walls are composed, manifest in the course of

* *Vide* particularly the Allgemeine Anatomie of Henle, s. 550, and Tab. v. fig. 26.

† There is every reason to believe that blood discs are developed in considerable numbers in the lymphatic vessels: the albuminous fluid which they contain is fitted, we must presume, to prove the matrix or blastema of these important cells, and they are evolved in it probably as a matter of course, their formation and evolution being assisted by the elaboration which it undergoes in the lymphatic glands. I cannot, however, regard this part of the office of the lymphatic system at large as otherwise than secondary, and in some sort adventitious, to that which is advocated for it in this paper.

their evolution, a special affinity for the watery element of the blood; and abstracting this from the fluid that has been shed into the areolæ of the body by the capillary arteries, they in the same proportion effect the inspissation of the current that is returning to the heart in the veins, and thereby endow it with powers of imbibition or endosmosis fitted to cause a ceaseless and rapid change in the plasma which is transuding the walls of the arterial system at every point.

The assumption of the chyle by the lacteals, or lymphatic vessels of the intestines, would seem to be mainly effected in the same way as is the lymph by the lymphatic vessels of other parts of the body; but unquestionably with a greater amount of penetrative force here than elsewhere. The lymphatics of the intestines begin, as they do everywhere else, in the form of cæcal sacs or canals, the only difference observable between their structure and that of the lymphatics generally, appearing to consist in the higher development of the essential element of the system—the nucleated cell—which is here conspicuous. The absorption of the chyle is readily to be explained in this way: The substance of the intestinal villi where they are met with, that of the spongy tissue which still forms the inner layer of the small intestines where they fail, like the substance of other tissues, is in a state of perpetual growth and decay. The decay here, as in glandular organs generally, takes place in the interior, towards intercellular channels or ducts—in the case under consideration, towards the interior of the lymphatic or lacteal vessels. But the cells of which the intestinal villi and pulpy inner membrane of the bowels principally consist, being evolved in immediate contact with the chyme, are penetrated by some portion of this (the elaborated chyme of the intestines being to them in some sort what the plasma sanguinis is to the tissues of the body at large), they rapidly attain maturity, and then they give way and discharge their fluid contents, mingled with a peculiar granular matter, into the ducts with which they are in relation. The fluid contents are the chyle,

the ducts are the lacteals.* The contents of the nucleated cells of the intestines, it is to be observed, are only white and milky when there is chyme, and that even of a certain description, present in the intestines; when there is none of this chyme present, their contents are transparent and colourless, or nearly so, as is also the product of their maturity and rupture, then entitled lymph, no longer chyle. Chyle, indeed, only differs from lymph in containing a relatively larger proportion of albuminous, granular, and fatty matter, which is exactly what might have been expected in the fluid that is bringing fresh pabulum for the use and maintenance of the economy, albumen being the representative of the azotized, as fat is of the non-azotized, elements of the animal body.

I only pause here to observe that, notwithstanding the mass of fresh material with which the lacteals are charged, they still transport a fluid of much less density than the liquor sanguinis, and are, therefore, in a condition to execute the important office which I have assigned to the lymphatic system generally.†

The anatomical distribution of the lymphatic vessels is in

* Doellinger appears to have been the first who approached the truth of this matter. He held that the villi of the intestines incessantly reproduced on their exterior or intestinal aspect by the aggregation of particles of chyme, in the same way as the germinal membrane of the embryo grows by the apposition of particles of the yolk, underwent in the same measure solution on their interior, and by this process formed the chyle of the lacteals. This is the germ of an idea that has proved most fortunately prolific since Doellinger wrote (*vide* Froriep's Notizen, Bd. 1, Nov. 2, 1828). Had he but viewed the aggregations of nucleated cells which compose the intestinal villi as derived from the common nutrient fluid, he would have anticipated Purkinje in his beautiful views of the intimate nature of glandular secretion. (*Vide* Versammlung der Naturforscher und Aertzte Prag. 1837, in Isis von Oken, No. 7, 1838, p. 573.) Views that have very recently been most satisfactorily extended and variously demonstrated by Mr Goodsir. (*Vide* Transactions of the Royal Society of Edinburgh, vol. xv. p. 295, 1842.)

† *Vide* the analysis of lymph and of chyle procured by Mr Lane from the same animal at the same time, by G. O. Rees, M.D., F.R.S., in *London Medical Gazette;* and Mr Lane's Article, 'The Lymphatic System,' in Todd's Cyclopædia of Anatomy and Physiology.

complete harmony with the views now taken of their vital function. In conformity with the principle of organs being found near to the places where their office is required, the office of the lymphatics must be general, inasmuch as the system is general. Mr Lane has, however, called particular attention to the fact that the lymphatics are by no means minutely ramified, like arteries, veins, and nerves, to every constituent particle of the body. They are never seen, save as vessels of a certain magnitude, that may be followed from the glands of the arm-pit and groin downwards, with the naked eye, by means of the knife and forceps to the very extremities of the fingers and toes. They run under the skin and in the intermuscular spaces, in lines more or less parallel to one another, and for long distances without receiving any lateral branches, although they frequently inosculate. Instead of growing larger continually, and from twigs and branches becoming trunks, in their upward course, like veins, the principal lymphatic channels on the dorsum of the foot, and on the back and palm of the hand, are actually seen dividing into great numbers of but slightly smaller canals, which then proceed upwards, and only come together again as they approach clusters of lymphatic glands, in which alone are they seen distributed like the blood-vessels into branches and twigs successively smaller and smaller, and then from twigs to branches successively larger and larger, till they emerge and resume their former fascicular distribution. The object here is evidently to *diffuse*, not to concentrate the influence of these vessels. The lymphatics of the extremities, in a word, have everywhere the same character, and bear the strongest possible resemblance in their disposition to the tubular drains of modern agriculture; and they have, in fact, something of the same duty as these to perform. It is only on the serous membranes surrounding the viscera that the lymphatics have rather more of the appearance of veins in their mode of distribution: still, even here, they are not in general seen ramifying extensively, and dividing into branches and twigs suc-

cessively more and more minute, until they escape the eye, except on the under surface of the liver; they rather proceed in a fasciculated manner, like bundles of rods. Speaking of the lymphatics of the viscera, Müller says expressly: "I am acquainted with no absorbent vessel that is not visible to the naked eye" (Physiology, p. 282); a conclusion that is borne out by the later researches of Mr Lane (Cyclopedia of Anatomy, Vol. III. p. 217).

It is now very generally agreed that the lymphatics are only disposed in the spaces between, and within the substance of, organs; a few canals may also accompany the larger blood-vessels of the viscera for a certain way on their destination; they do not appear to penetrate the substance or parenchyma of any organ. The mercurial injections of Fohmann, Panizza, and others, made by pushing a pipe at random into the areolar tissue and parenchyma of organs, are acknowledged by the best anatomists to be injections of the interorganic areola. The vicinity, and not the immediate presence, of lymphatic vessels, therefore, suffices for the economy; and this accords with the views of the office which I have advocated for them.

The same views seem to acquire additional and peculiar support from the singular development of the lymphatic system which we observe in tortoises, lizards, and serpents—animals in which the common integument is obviously unfitted for the elimination of water or watery vapour, and in which some substitute for this was therefore necessary. In these creatures, the lymphatic system may be said, without exaggeration, to be enormously developed; it is here of such extent and capacity that it is actually furnished with contractile sacs or hearts in various situations more or less remote from the origin of its constituent canals, for the propulsion of their contents towards the central organ of the circulation. Whether there be a corresponding increase in the number and magnitude of the lymphatic vessels in those higher animals which are covered with a horny or impervious integument,

such as the Manis and Armadillo, I do not know, and have no opportunity of ascertaining, but I should anticipate that this was the case.

It is also important to observe that in certain diseases where the function of the skin is seriously implicated, as it is in anasarca, the lymphatic system acquires its highest state of development. To display the lymphatics, anatomists commonly select the body of one who has died of some disease complicated with general dropsy.

The relationship of the lymphatic system at large to the capillary circulation, and its subserviency in the direction already indicated, are still further proclaimed by the manner in which it is finally connected with the blood-vessels. The watery fluid appropriated by the lymphatics is not poured into the veins in the vicinity of the parts and organs where it is gathered; this would have had the effect of again attenuating the returning current, and undoing all that had been done.* The contents of the lymphatics are added to the blood only at the moment of its entering the heart—at the nearest eligible point, it may be said, to that organ, from whence it will immediately be sent to undergo exposure in the lungs, and, besides the heat which it then and there acquires and the change it undergoes, to lose so much water as will give the tide in the pulmonary veins a somewhat higher density than that in the pulmonary arteries, and so fit it to effect the drainage of the delicate tissue of the instrument of respiration.

* In some of the lower animals it would seem that all the lymphatics do not proceed to discharge their contents at the angle between the internal jugular and subclavian veins. This is held to be particularly the case in birds, in which several considerable lymphatic vessels have been described as joining the veins of the abdominal viscera. Lippi, who would have extended the same anatomical arrangement to man and the higher mammalia, totally failed to establish his views, when in contact with the distinguished anatomists of Paris. The blood of the vena cava ascendens, however, having parted with the urine and the perspiration of the whole of the inferior extremities, might obviously be very considerably diluted without losing its capacity to imbibe from the tissues through which it passes.

This, in fact, I hold to be the end of the pulmonary exhalation, an excretion, which, although it may be in some measure physically unavoidable, is still subservient to an important vital function.

As a final confirmation of my views in regard to the import and office of the lymphatic system, I would yet further refer to the singular amount of disturbance which ensues in the system generally upon any implication of the functions or structure of the serous membranes, which may be regarded in the main, I conceive, as contrivances for the accommodation of large numbers of lymphatic vessels;* for example, upon inflammations of these membranes, adhesions to any extent of their opposite surfaces, &c. The mischief in such cases is commonly ascribed to interference with the motions of the organs whose serous coverings are interested, to the extent of the surface affected, and so on; but it certainly depends on some cause of greater moment: I believe that the nutrition, that the life of the organ implicated, is compromised; the conditions necessary to the access of the nutrient fluid, and to the removal of the plasma having done its duty, and of effete matter, are interfered with, through implication of the function of the lymphatic vessels, and that thence arise the great amount of general constitutional disturbance first, and the constantly fatal effects in the end, which accompany affections of the serous membranes.

Thus far the function of the lymphatic system and skin has been considered as subservient to the needful interchange of the nutrient fluid, as this goes on through the spongy walls of arteries and veins. But it seems highly probable that the

* It is rather interesting to remark here that the only two of the abdominal organs which are not included within duplicatures of the peritoneum,—the pancreas and the kidney,—are organs whose secretion is extremely watery. The blood of the capillary arteries of these two glands losing so much water as it does to their respective secretions, the blood of the capillary veins is rendered so dense that it suffices, of itself, to effect the drainage of their tissues.

same function is efficient in no insignificant degree in facilitating the transit of the stream of blood still included in the interior of the capillary arteries and veins especially.

Cullen, as is well known, founded his Theory of Fever on the hypothesis of a *spasm of the extreme vessels;* and I conceive that the inductions here presented are calculated to throw a new light on this idea, and, it may be, even to invest it with new life—that is to say, if, escaping from the labyrinth of the groundless empiricism in which it is now wandering helplessly, the world be destined ever to see a rational theory of medicine again. Ever since the time of Cullen, and influenced doubtless by his great name, physiologists may be said to have been in eager quest of a cause for the motion of the blood in the capillary vessels other than that which is derived from the mechanical force of the heart; and various bold hypotheses have been advanced on the subject. The majority of the highest authorities in physiology at this time, however, recognize no moving power in connection with the circulation of the blood among the more perfect animals, save that of the heart.

In acceding to this view, which I have no hesitation in saying that I do in the main, it is still open to us to inquire whether or not there be any means by which the course of the blood, along its more minute channels especially, is *facilitated.* It were vain to deny that the amount of resistance encountered by a somewhat sluggish fluid like the blood, propelled through canals of the 20,000th and 25,000th of an inch in diameter, must needs be very great; and yet the rapidity and obvious ease with which we see it shoot through such canals in the transparent parts of living animals, drawn apparently rather than driven through them, seems to indicate that everything like resistance is annulled.

It is certainly a very remarkable fact that the column of blood contained within a fine vessel of a living animal does not fill it from side to side as an evenly-mingled mass of coloured and colourless corpuscles and transparent plasma. The

red discs are seen shooting rapidly along the centres and as if repelled by the walls of the containing channels; the colourless corpuscles, on the contrary, suspended in a film of plasma of very appreciable thickness, interposed between the serried column of blood-discs in the centre and the walls of the vessel, move on, rolling over and over in contact with the bounding parieties, and as if attracted by them. The disposition here is unquestionably one of much significance. "If the blood moves in the capillary vessels independently of the heart's action," says Müller (Physiol., 2nd ed., p. 236), "it must be by virtue of an attraction exerted on it by the walls of the capillary vessels, as supposed by Baumgärtner and Koch. But we cannot conceive how such attraction could aid the circulation of the blood. . . . unless it is again admitted that this attraction of the capillaries for the blood is exerted only while the blood retains its arterial character, and ceases when it has become venous." Now I apprehend that when the extent of the apparatus is contemplated that is provided for the especial purpose, as I maintain, of effecting a change in the physical constitution of the out-going and the in-coming currents of the circulating fluid, and a consequent ceaseless interchange of elements between these, in conformity with the laws of heterogeneous attraction or osmosis, we seem to have obtained that which was wanting to explain the remarkable phenomena of the capillary circulation: the walls of the arteries, from the sigmoid valves at the root of either of the great cardiac trunks, to the limits of their peripheral ramifications, are incessantly transuding the plasma that gives life to the several organs of the body, and are at the same time parting with a fluid of considerably less density than the plasma or liquor sanguinis, this fluid being either abstracted by the sudoriparous glands and dissipated, or appropriated by the vital act of the lymphatics and returned at the proper moment for the use of the economy. The veins, on the contrary, filled with a fluid augmented in density by the whole amount of the water that has been thrown off from the surface of the body,

or transferred to the lymphatics, are incessantly absorbing, or rather are incessantly being penetrated by, fluids from the tissues through which they pass. In this way are different capacities of osmosis continually engendered, — first, as between the blood vessels and the liquor sanguinis; and, secondly, as between the liquor sanguinis and the various organic tissues with their several affinities or aptitudes. The factors here are indeed extremely complicated; for besides those that thus immediately present themselves to us, there are the qualitative changes effected in the plasma itself,—now surcharged with life-giving power, and then contaminated by the effete products of vital acts accomplished,—changes that must go for something in the wonderful processes which build up, and for a time maintain in its integrity, the animal organism.

OF THE SPLEEN

AS AN ELEMENT IN THE LYMPHATIC SYSTEM, AND HAVING A LOCAL FUNCTION OF THE SAME SPECIFIC NATURE AS THAT OF THE LYMPHATIC SYSTEM AT LARGE.

Before concluding what I had to say on the subject of the Lymphatic System, I take occasion to advert to that mysterious organ the Spleen, upon the function of which I ventured a new hypothesis in a note to the appropriate paragraph of my translation of Wagner's Physiology. That the spleen is most intimately connected with the lymphatic system—is indeed an element in its constitution—seems demonstrated by the fact that

the organ is only encountered in those classes of animals where the lymphatic system exists: where this is wanting, the spleen too is absent: where it is largely developed, the spleen is of ample size. In the note to Wagner referred to (p. 328), after commenting on the Pancreas as a large salivary gland existing for the purpose of diluting the pultaceous mass, the product of digestion completed in the stomach, on its passage into the intestines (ib. p. 327), and remarking on the fact of the singular rapidity with which fluids are absorbed from the stomach by its veins, I go on to observe that the situation of the spleen, its connection with the stomach and portal system (and, I should have added, its anatomical structure and intimate relationship to the lymphatic system at large), seem to give a key to the nature of its function. The rapid absorption of water by the gastric veins must, upon occasion, be accompanied by a great degree of attenuation of the blood they contain, which blood, sent immediately to the liver, would be apt to transude the parenchyma of that organ with excessive facility; but the blood of the veins of the spleen, having undergone a very considerable degree of inspissation through the action of the lymphatics that make up so large a portion of its structure, the much diluted blood of the gastric veins, mingled with the greatly inspissated blood of the splenic veins, composes a fluid of medium density, fitted for circulating through the liver. The spleen, therefore, I hold to be no diverticulum or reservoir for the blood when the process of digestion is not proceeding, as is so commonly presumed—a notion which to me has the look of a last resource to puzzled physiology—but an organ having an immediate relationship with the appropriation of liquids for the use of the economy by the stomach, and the safe performance of the circulation of the liver. The situation of the spleen, its existence as an element in the lymphatic system, its anatomical structure, and the analysis of the blood of its veins, all appear to me to indicate, as they all seem to supply, satisfactory evidence of the part it plays in the

animal economy. In structure, the spleen consists of little more than a congeries of blood-vessels and lymphatics. The volume of its arteries and veins, relatively to its bulk, is immense, and nowhere else in the body (unless it be perchance in the relatively insignificant conglobate glands), do lymphatics exist in such large proportion as in the spleen. Now these lymphatics must needs perform precisely the same function as those of the body at large: they secrete a simple watery lymph, and secure within the sphere of their influence the first condition necessary to the engenderment of the osmotic force. They are, in fact, the coefficients of the secerning elements and efferent canals of secreting glands generally. But the secretion of the spleen, like that of the lymphatic system at large, being a simple attenuated albuminous fluid—containing no proper excrementitious matter requiring to be purged from the body—is only temporarily withdrawn from the circulation. The end of its abstraction accomplished, the fluid is returned by the thoracic duct to the heart for the use of the economy.

The reliable analyses that have been made of the blood of the splenic veins show how large the quantity of water taken up by the lymphatics of the organ must be. To cite but one of these, that of Beclard, as quoted by Dr Carpenter (Op. cit., p. 169), whilst arterial blood was found to contain 221·1 of solids, splenic venous blood gave as many as 253·5 of solids per 1000 parts—32·2 per 1000 more of solids, consequently, in the venous than in the arterial blood. The spleen and pancreas may be regarded as organs mutually complementary, but in harmony precisely by the opposite natures of their several functions—the spleen having reference to the first stage in the process of alimentation: the assumption of solid and liquid food; the pancreas to the act of digestion completed.

Müller attained to the truth, in part at least, when he said (Physiology, p. 620), that "the function of the spleen probably consists in the production of some change of unknown

nature in the blood which circulates through its tissue." One important item in the presumed change now made known, and the influence of this change explained, we may, as it seems to me, conclude with some assurance of certainty, that the spleen has neither anything to do with sanguification as has been supposed, nor with serving as a cistern or diverticulum for a mass of blood at times when digestion is not going on. When nature requires such a reservoir, as she does when another important function, Respiration, is not proceeding, such being the case in the great families of the phocidæ and the balænidæ, she provides it in immediate connection with the circulating system itself. That the spleen can have nothing to do with such an important process as sanguification, is demonstrated by the single fact that the organ may sometimes be extirpated without immediately fatal effects following. The office, however, with which I would connect the organ is a secondary though, for the well-being of the individual, probably a sufficiently important one; and I venture to believe that the sudden deaths to which animals that have survived extirpation of their spleens have been found obnoxious, were due to *hepatic* rather than *cerebral* apoplexy, to which they have been ascribed.

ON THE INFLUENCES

PRODUCTIVE OF INTERMITTENT AND REMITTENT FEVER

AND ON THE

PATHOLOGY OF SCARLATINA,

IN CONNECTION WITH

THE VIEWS EMBODIED IN THE PRECEDING ESSAYS.

When M. Fourcault made the remarkable discovery that a healthy animal very soon dies if its body be covered with an impervious glaze; and Messrs Becquerel and Breschet, repeating his experiments, observed that the extinction of life under the circumstances indicated was accompanied by a signal fall in the temperature of the body, none of these distinguished physiologists offered any explanation of the phenomena they witnessed. These, if we may infer so much from their silence, appear to have been altogether enigmatical to them. Messrs Becquerel and Breschet only see the old office of the cutaneous exhalation as a means of refrigerating or regulating the temperature of the body to be gone. Here was the exhalation completely suppressed, and the temperature, instead of rising as it ought to have done according to accredited notions, began at once to fall, and went on sinking till life was extinct, when it was found but 3° Centigrade higher than the temperature of the surrounding atmosphere. The purpose of my paper on the Function of the Sudoriparous System of Glands, in which I venture on an explanation of the

phenomena observed by the French physiologists, being exclusively physiological, I have scarcely hinted at any practical application of the views there embodied ; but as a brief survey in this direction may possibly present these views in the same interesting and important bearing in which they meet me, I here supply what I imagine might fairly be regarded as a deficiency in my Essays. It is indeed only when the sudoriparous system comes to be regarded as so intimately connected with vital manifestation, as it unquestionably is, and a reasonable explanation is given of the way and manner in which it is so, that we can understand wherefore such serious consequences follow all implications of its function. Nay, it is only when we discover that this system is of a nature as *essentially*, though not so *immediately*, vital as Respiration, Circulation, and Innervation in man and the higher animals, that we begin to see its function, not only as among the first factors in health but in a vast variety of diseases—and these generally of the most serious import—to which man in especial is liable. It was not without good grounds that Dr Moseley* long ago remarked that "Cold is the cause of almost all the diseases of hot climates;" and that Dr Mouat† of the 13th Light Dragoons, informs us that "of 3394 cases of disease of all kinds treated in the Regimental Hospital of Madras, 1372 of the patients ascribed their illness to *cold*, and only 62 attributed theirs to exposure in the sun, i. e. to *heat*." But we have not to go to the tropics for an assurance of the pernicious effects of cold upon the human body : even in temperate lands, cold, i. e. implication of the function of the cutaneous glands, is the acknowledged cause of the greater number of the diseases to which their inhabitants are subject. The rationale of this fact it is true has not yet been supplied ; but as soon as we perceive that he who has suffered a *chill* has had the

* On Tropical Diseases, London, 1804.

† Quoted by Sir J. R. Martin, in his work on the Influence of Tropical Climates, p. 89.

seereting faculty of his skin deranged, or suppressed in a greater or less degreo, and is brought into precisely the same condition as he would be were his body more or less completely covered with an impervious varnish, and when we are aware of the fatal effects of such an application, we are at no loss to aecount for all the mischief that follows: a proeess indispensable to the continuanee of vital action has been interfered with, and disturbance of the general health, eommensurate in degree with tho amount of derangement in tho particular function implieated, forthwith ensues. In tropical countries, and in the summer of more temperate latitudes, when the heat approaches or equals that of the tropies, *fevers* of various type are the usual eonsequence; as in temperate lands and in the winter season inflammation in various forms and affeeting different organs is the common sequel of exposure to cold.

The implication of the sudoriparous function through the influence of cold is very probably of the same essential nature as that which we observe when we have tho transparent part of a living animal under the microseope, and do it an injury. On prieking the web of a frog's foot, for instance, with a fine needle, instantly all motion around the injured part is arrested: the blood stagnates in its channels, which dilate visibly, and by-and-by the blood-eorpuseles run together and form aggregrated masses. The explanation of this that has been suggested is simple and satisfaetory: The local injury impresses the percipient eentres in a way that indisposes or incapacitates them from transmitting the usual stimulus upon which depends tho tone of the living tissuos; these are consequently paralyzed, and then follow stasis of the blood in the blood-vessels and all the phenomena of inflammation. But the influence of cold suddenly and more or less extensively applied to the surfaee of the body, is probably little different from that of more mechanical eauses of morbid action. The effect of eold on the peripheral nerves is paralyzing to the excito-motory system; and, eonnected with this paralysis, are suspended function and more or less of more local or more general de-

rangement—in a word, disease. In this way it seems may we render a satisfactory account of the influence of cold in the production of disease not only in all the colder and more temperate but also in all the hotter regions of the earth.

One of the problems of highest interest in medical science that waits for solution at the present moment, is perhaps this: *To explain the cause of the unhealthiness of so many intertropical climates, and of alluvial flats and marshy districts generally in temperate lands where the summer temperature is high.*

A particular specific poison or contagious influence designated *malaria*, as is well known, has been invoked, and is very generally admitted as a means of accounting for the fevers in especial, which prevail in the lands and localities circumstanced as above. But after a very careful study of this interesting subject, and influenced doubtless by the views I take of the function of the sweat-glands, and my interpretation of the facts connected with exposure to excessive degrees of heat, I confess that I entirely concur with those observers who, with ample experience in tropical countries and fever-engendering districts, have felt themselves authorized to deny the existence of any specific miasm or malaria as the source of fever. The product itself, as always said, of vegetable matters in a state of decay under the combined influence of heat and moisture, it might be expected not to escape the keen scrutiny of modern chemical analysis. Yet has it never been demonstrated. And then we are familiar as chemists with the products of the decomposition of vegetable matter under such circumstances, and, as physiologists, with the effects upon the animal economy of the gases and other compounds evolved, and these certainly bear no resemblance to the phenomena of remittent and intermittent fever.

According to my ideas all the ill effects ascribed to malaria may be interpreted upon general physical grounds. Among the number of these I should particularly specify Topographical situation, Thermal states of the air, and Hygrometric states of the air. (A.) *Topography: position:* low-lying flats, and

plains more or less elevated above the sea-level, with little or no large vegetation in the shape of trees, and from which radiation is consequently quite unimpeded. (B.) *Thermal state of the air :* (*a*) where the temperature both of the day and night is excessive (78°—85° F.) ; (*b*) where the day temperature is excessive (78°—85° F.), and the night temperature is relatively low (55°—65° F.). (C.) *Hygrometric state of the air :* (*a*) where, with like excessive temperature both of the day and night, the air at all times approaches hygrometric saturation ; (*b*) where, with an excessive day temperature, and it may be dry air, the night temperature falls low and the air becomes relatively moist, or approaches saturation.

I am the more emboldened to criticise and call in question the common and accredited views on the subject of malaria, from observing the discrepancies and contradictions that prevail in the writings of our highest authorities on the diseases of tropical climates; the result, as I believe, of the want of true physiological principles as guides to accuracy of observation, first; and next, of logical and legitimate inference from the facts that are known. I turn to the pages of Sir J. Ranald Martin's exhaustive treatise "On the Influence of Tropical Climates in producing the Acute Endemic Diseases of Europeans" (2nd ed., Lond. 1861), and find that while advocating the usual theory of the cutaneous exhalation as the special means contrived for regulating or reducing the heat of the body, he is yet doubtful of its constantly salutary effects. The European arriving in the climate of Calcutta feels the heat extremely, as matter of course, and his internal temperature tends to rise, but, says Sir Ranald, "nature opens the sluices of the skin, and by a flow of perspiration reduces the temperature to its proper standard;" he, however, adds immediately afterwards—"We must not conclude that this refrigerating process adopted by nature to prevent more serious mischief, is itself unproductive of any detriment to the constitution, when carried to excess." The action of the skin is therefore at one time seen as salutary, and at another as deleterious. A little

farther on than the page from which the above quotation is taken, Sir Ranald expresses himself thus: "For good or for evil, as it may be, the function of the skin is highly important." There can be no doubt of this; but how it is so, is not explained by the statement that such is the fact; and as a physiologist I should say that the function with which the skin, as regulator of the bodily temperature, is here endowed, were it the true one, could never be evil, but, as it was necessary, so must it always be good. The limitation at the end of Sir Ranald's sentence, "when carried to excess," implies diseased not healthy action, and so removes the question from the category of physiological consideration.

On the main subject of malaria, again, Sir Ranald is scarcely more decided; he seems even at times disposed to give up this hypothetical maleficent influence altogether. Quoting Dr Heyne "On the Hill-fevers of the Southern Peninsula of India," who, after speaking of certain ranges of hills "which have rendered themselves known to Europeans for the malignity of the fever that prevails there," and certain other ranges "that are as constantly free of hill-fever," goes on to say, that "the received opinions as to the vegetable or marshy origin of fever will not hold, for the hills in question are not more woody than other healthy places, and some indeed, where the fevers were most destructive, are quite naked of trees." Dr Heyne, driven from the malaria theory, is himself disposed to associate the unhealthiness of the destructive districts with the nature of their mineral constitution. The hills where fever most prevails, he informs us, are of granite; and besides the quartz, felspar, and mica which make up their mass, contain "much ferruginous hornblend," a mineral in which Dr Heyne says he "supposes the cause producing fever to reside."

Sir Ranald Martin is himself disposed to indorse this view. He quotes many authorities for the connection of fevers of bad type with ferruginous soils; he has even had the soils of feverish lands and more limited districts forwarded to him for analysis, and seems always to have found them largely ferru-

ginous. Iron, however, save when particles of it are taken into the lungs with the dust of millstone grit, &c., is no poison to the human body; on the contrary, it is a necessary constituent of the blood, and stands in the foremost rank of tonic medicines; so we may I think without scruple abandon the ferruginous origin of fever without detriment to pathology. Dr Kirke, indeed, of the Bengal army, who is next quoted by Sir Ranald Martin, passing by unnoticed the ferruginous theory, attributes the fatal fevers he had to deal with during Sir Charles Napier's administration of the affairs of Sindh, to "exhalations from limestone rocks;" and Sir Wm. Napier, the historian of his distinguished brother's career in India, is of opinion that "the fever and other epidemics which prevail at irregular periods in Sindh, arise from exhalations produced by volcanic action; the country, though alluvial, being so subject to earthquakes, that in the course of 1819 nearly the whole surface of Cutch was changed."

Now carbonate of lime is in itself a perfectly harmless substance—as witness the good health of those who, in other lands, live upon limestone hills and over chalk formations, and who are engaged in quarrying limestone in various states of integration, whether as Carrara marble, as Kentish rag, or as chalk, and even in burning it into quicklime. And then we have no information in regard to the nature of the presumed exhalations which this rock produces; we should not scruple to maintain that limestone was about the last rock on the face of the earth likely to throw off exhalations of any kind, least of all exhalations productive of disease.

What need we say of Sir William Napier's theory, in fine? It is the theory of a non-medical man indeed, and its author may be held the more excusable on this account for giving it birth; it is at least as satisfactory, if it be not more satisfactory, than any of the professional hypotheses. We are quite familiar with the exhalations of volcanoes and volcanic districts, and these, taken in by the lungs, are deadly enough assuredly; for who can breathe carbonic acid gas and

sulphurous vapours with impunity? But these stifle a man; and in a sufficient dose end the business quickly; they do not produce the phenomena of remittent and intermittent fever.

The source of malaria, the cause of fever according to generally accredited views, is vegetable matter decaying under the united influences of heat and moisture; but I find Sir Ranald Martin, moved doubtless by what his own excellent powers of observation suggested to him in the course of his ample experience in India as unsatisfactory in the malaria theory, putting the following query:—"May not the decomposition [of vegetable matters] by the iron [in ferruginous soils], together with the magnetic phenomena elicited by heat and other agents, be productive of disease and of fever especially, in certain climates and localities?" I forbear criticism on this sentence, as the writer himself offers a sort of apology for its introduction; and only direct attention to the shifting of the ground from a position felt to be untenable to another admitted to be in the realm of pure conjecture. Sir Ranald, however, is in good company in his conjectures. The late distinguished military surgeon, John Hennen (Pr. of Military Surgery, and Med. Topog. of the Mediterranean, Lond. 1830), still clinging to the doctrine of malaria, but doubtful of its source or the cause of its production, observes that "whatever be the cause, it is certain that in many countries the malaria does not arise until all the surface-water has disappeared, and leaves the whole face of the country, including the courses of the winter streams, an arid desert." This observation is made, if my memory serves me aright, in connection with the fact that the soldiers of the British army in Spain, when exposed on the arid plateaus of that arid land, suffered much from fever; the source of the malaria, to which the disease was as matter of course ascribed, being held to be the cracks and fissures with which the ground was rent in all directions by the heat and drought.

A clever writer in a late number of one of our pleasant

periodicals,* gives us an idea of the kind of country frequently encountered in Spain, which we may presume presents the general features of that in which our soldiers suffered so much from fever. "As far as the eye could see—and in La Mancha the eye can see very far—a rolling prairie of reddish-brown, lay baking into brickdust under a powerful sun. There was not a tree, bush, or green thing within the limits of the horizon to hint the possibility of shade or moisture, nothing but parched thistles, sparse stubble, glare, heat, and drought." This is precisely the land on which the oppressively hot day is followed by the piercingly cold night, in virtue of the free radiation that succeeds the disappearance of the sun, and in which exposure without adequate covering is almost certain to be followed by disease.

More distinguished as an authority than any of our English army surgeons is Alexander von Humboldt,† who, speaking in his personal narrative of the missionary villages of the Aturès and Maypurès, situated near the great cataracts of the Orinoco, and of their desolate condition through the prevalence of fever, informs us that these villages are located on the bare granitic plateaus that border the great river, and that the surface of the rocks is smooth, black, and as if coated with plumbago; without any possibility, therefore, of the presence of decaying vegetable matter. The missionaries and natives assert that these rocks give off unwholesome exhalations, so that it is especially dangerous to pass the night upon them. Humboldt himself, after referring to the chemical analysis of the black crust, which is of extreme thinness, and consists of oxide of iron and manganese, asks whether "it is possible that under the influence of excessive heat and constant humidity, the black crusts of the granite rocks are capable of acting on the ambient air, and producing miasmata, with a triple base of carbon, azote, and hydrogen?" To the query thus put he himself responds with a "J'en

* Cornhill Magazine, April, 1867.

† Voyages, &c., chap. xx. vol. ii. p. 299.

suis douteux." It might, however, have been replied, that such a thing were possible; though when we observe the great naturalist himself referring in another place to the thermal and hygrometric states of the atmosphere in connection with the excessive heat of these smooth black rocks—for the thermometer laid on them rose in the day to 118·4° F., and in the night did not fall lower than 96·8°—we do not find ourselves reduced to the necessity of seeking for the insalubrity of the rocks and the districts in which they occur, in any hypothetical compound of carbon, hydrogen, and azote. A human being laid on such a heated surface with a covering over him, is forthwith in a vapour-bath of the temperature of his body, in which we know that he cannot exist for even an hour without suffering in his health; or, exposed without adequate covering, his body speedily loses heat by radiation to such an extent that though the atmospheric temperature be as high as 75° F. or more, he experiences a *chill*, and is then, and under such circumstances, it may be said all but necessarily attacked with fever.

It is possible, as we know, to remain for a certain time in the atmosphere of a stove excessively heated (200°—260° F.); and if the stay be not protracted beyond 15 or 20 minutes, no inconvenience is felt beyond some excitement of the circulation, which gradually subsides. If the heat of the stove were about or but a little higher than the temperature of the body, and the air perfectly dry, it seems uncertain how long exposure in its atmosphere could be borne without detriment to health. All we know warrants the conclusion that the phenomena of fever would not result under such circumstances, though those of insolation or sun-stroke would probably ensue. In the dry-air stove there is the freest possible action of the sudoriparous glands; the capillary arteries then exude and the veins imbibe with great rapidity, and the nutritive and vital acts are even performed with a kind of increased energy. They are so performed however only for a limited time; excitement begets exhaustion, which is but another name for incipient

disease; and circumstances continuing unchanged, more positive disorder of some internal organ is not long of showing itself, and bringing the life of the individual into jeopardy. Now the atmosphere during the dry season over the greater part of the peninsula of India, to cite a particular instance, may be fairly enough likened to that of a dry-air stove; and we have seen some of the authorities on Indian disease assuring us that exposure to heat is by no means the most common cause of disease;—it is cold that is the enemy. Under ordinary circumstances of exposure this is undoubtedly the fact: in the civil service, and during the usual routine of military duties, men do not suffer very immediately from the heat; but under extraordinary circumstances the case is different, as witness what happened to a convoy in its passage across the desert of Scinde in the hottest month of the year, of which we have a most graphic account from the pen of Gen. Sir T. Seaton.* In the course of no longer a time than seven days, six out of fourteen Europeans, more than one hundred sepoys, and many more than three hundred camp followers, were lost from sun-stroke, fever, and cholera. In the first part of the march "the heat," says Sir Thomas Seaton, "by day in the close jungle was intense and stifling, and the heavy dews by night chilled us to the very bones; and during these short marches over a distance of only forty and a half miles, were sown those seeds of disease that subsequently grew and ripened into death." The jungly district passed, the proper desert was entered on: "Day dawned upon a frightful waste,—a boundless plain of hard alluvial soil, apparently deposited by the annual overflow of the Indus. Not a tree, bush, shrub, or blade of grass was to be seen,—nothing but a scene of dreary desolation. The heat in the tents rose to 119°, the whole camp smelled like a charnel-house, and in very truth it might be called one, for no person could take three steps anywhere without seeing a dead or dying man or animal." It were as little satisfactory to recur to any hypothetical agency

* See his work: From Cadet to Colonel, 2 vols., London, 1866.

as a means of accounting for these disasters, as it is easy to render a reasonable account of their occurrence when the vital nature of the sudoriparous function is understood, and the atmospheric conditions indispensable to the continuance of human life are known.

So much for the effects of exposure to excessive heat, abstracted as much as possible from the influence of the lower grades of temperature that are necessarily felt during a campaign and in the field.

Let us now consider the effects of a high temperature with the concomitant of a *moist* instead of a dry atmosphere. Reverting for a moment to the remarkable observation of M. Delaroche, who found in his second series of experiments, that he himself could not bear exposure in a vapour-bath a degree or two Cent. *lower* in temperature than that of his own body for 20 minutes without extreme distress, and that animals exposed, under similar circumstances, died within a very short period, and placing these facts in connection with the views that have been taken of the vital nature of the sudoriparous function, we are at no loss to account for all that happens when extreme moistness is associated with extreme heat of the atmosphere. In an excessively hot, dry atmosphere, death ensues when occurring suddenly or rapidly, from the effects of excitement and such a rise in the temperature of the body as is incompatible with life. In a hot, moist atmosphere of even no higher temperature than that of the animal body, death ensues precisely as it does when the entire surface is covered with an impervious glaze or varnish: the conditions indispensable to the access of oxydized plasma and the removal of deoxydized plasma are wanting, and life ceases as matter of course.

Now it is highly interesting and important to observe that the atmosphere of the most unhealthy intertropical climates known, differs but little from that of a vapour-bath between 80° and 90° F., and that the dew-point of the air in these countries is for the most part no more than a few degrees—

often no more than a single degree, below the temperature of the ambient medium. Were the temperature of the air between 90° and 100° F., and the point of deposition in the same proportion high, man could not by his constitution continue to exist for more than an hour at most. In a country having a high mean temperature (from 78° to 85° F.), therefore, and an atmosphere close upon the point of saturation, which is precisely that which obtains on the west coast of Africa, with its hundreds of miles of mangrove swamp and marshy flats, in the Soonderbuns of the Ganges and in all the jungles and low-lying lands of India at particular seasons of the year, man is on the verge of circumstances that are even incompatible with his existence. He has but to be exposed to fatigue and the direct heat of the sun to be brought into such circumstances. The hot and all-but saturated atmosphere then acts precisely as does a confined vapour-bath: the wants of the system in its state of excitement, and requiring the freest access of the most thoroughly oxygenated plasma to keep up movement and life in its several constituent atoms, are interfered with: i. e. *the heat brought by the blood in the arteries is not converted into motion whilst passing from the arterial to the venous state*; endosmotic action ceases, great general derangement of the health—*Fever*—ensues, and life is too commonly the forfeit.

Besides malaria as the speculative cause of disease in hot and intertropical lands especially, some writers are found speaking of a certain *sol-lunar influence*—a mysterious maleficent power emanating from the sun and moon, independent of the heat and light which these great globes give forth, and affecting the human system in several disastrous ways. If this idea be more than a remnant of the belief, once universal, in planetary influence upon the health and destinies of man, still prevalent among the illiterate and half-educated masses of society, and having its organs in Moore and Poor Robin, I can find no satisfactory scientific evidence of any grounds for it in fact. The sun changes in right ascension and declina-

tion, and influences the weather and the seasons most markedly, and through the weather the health of the human being; the moon changes incessantly in latitude and longitude, and, between one moment of her circuit and another, presents to us different proportions of her illuminated disc. She certainly influences the ocean-tides remarkably; but that she influences the weather in an appreciable manner is not borne out by any accurate observation. The sun, as lord paramount of the sky, in connection with the state of the earth's surface as regards wetness or dryness, nakedness or clothedness, whereby the atmosphere in turn is influenced, is the sole and all-sufficient agent in whatever most immediately influences the weather and the health of man. The solar influence, apart from hygrometric considerations, is particularly manifested by sun-stroke, in the way so well described by many writers, i. e. by congestion of the brain, a state which is so apt to pass into acute inflammation of the substance or investing membranes of that organ. Lunar influence again seems to be but another term for the effects of that fall in the temperature of the night season, in connection with the relatively increased moistness of the air which then ensues: the air loses several degrees of heat, and in the same, or nearly the same, proportion does it gain in degrees of humidity. Growing relatively to its temperature more and more moist, it is soon in a state in which it can not only dissolve no more watery vapour, but in which it begins to deposit what it already held in solution; hence the heavy dews, and the mists or earth-clouds of unhealthy intertropical climates, and of alluvial and marshy temperate lands generally; hence also the deleterious effects of exposure to the night air upon the frame of man, effects particularly felt when the moon is shining brightly, or shows a ruddy face—i. e. when the sky is clear of clouds and radiation is most free.

With the views which have guided us throughout these papers in regard to the vital nature of the function of the skin, all that was mysterious and unintelligible in the doc-

trines of malaria and sol-lunar influence seems to disappear, or to merge into what is perfectly natural and comprehensible. A chill, in other words a sensible abatement in the heat of the surface, with implication in a greater or less degree of the sudoriparous function, is the almost invariable precursor in nearly the whole of the serious inflammatory and febrile diseases to which the body of man is obnoxious. The remarkable fact first ascertained by that most able and conscientious observer, Dr John Davy,* being now put beyond the sphere of doubt, viz., that the temperature of the body sinks, under the influence of exertion, instead of rising, as sensation seems to indicate, it strikes me that we are still further freed from the necessity of recurring to unknown and hypothetical agents as causes of the diseases which arise under exposure to atmospheric inclemencies, whether of heat or cold, especially when such exposure is conjoined with fatigue. On setting out on a journey in the island of Ceylon, Dr Davy found the mean temperature of his palanquin bearers to be 98·9° F. under the tongue; calling a halt at different times as he proceeded, he ascertained it to be successively 98·6° and 98·5° F. Doubtless had he pushed these men beyond their powers, their heat would have fallen still lower, and, with anything like exposure in the night season, he would have had them down with fever. The recent researches of philosophers into the co-relation of the physical forces place this subject in a very striking and appreciable point of view. By motion we readily produce heat; and conversely by heat we as readily produce motion; but in either instance the first factor is absorbed by, or turned into, the second. Mr Joule demonstrated that the motion of 772 lbs., through the space of one foot, will raise the temperature of a pound of water to the extent of one degree of Fahrenheit's scale; and conversely the expenditure of one degree of Fahrenheit's scale is competent to move 772 lbs. weight through a space of one foot.† We cannot take a single step

* See his Researches, Anatomical and Philosophical, vol. i. p. 199.

† Joule On the Mechanical Equivalent of Heat (Philos. Trans. 1850 and 1852).

without expending some portion of our bodily heat; we cannot take many steps, i. e., we cannot undergo fatigue, without expending a much larger amount, for the supply of which the breathing is accelerated, and the pulse increased in force and frequency; precisely as in the locomotive, with increased speed, i. e., greater expenditure of motion, there is a larger consumption of fuel, and a quicker stroke of the piston. As the amount of motion to be had from the locomotive, again, is mainly to be measured by its capacity to consume fuel, i. e., by its capacity to supply itself with heat; so is man's immediate power to undergo fatigue in the ratio of his power, through the medium of food and wholesome air, to engender caloric. The fuel failing in the tender of the locomotive, motion comes to an end, even as a man an hungered lays him down to die, and as an army in the field without a commissariat is an army encumbered with the dying and the dead. Our great Duke appears to have taken little thought about fighting his battles in comparison with making sure that his soldiers had their rations. Dr Davy himself, in connection with the facts he noted, observes on the instances of sudden death that have occurred from taking a copious draught of cold water, or from plunging into cold water after exhausting fatigue, and when the body is commonly said to be heated. In such cases, and in harmony with all that is now known to us as philosophers, the temperature of the body was then below the natural standard, and the sudden abstraction of a few more degrees of heat by the cold water, whether taken into the stomach or applied to the surface of the body, brought the blood to a point that was incompatible with its power as vital motor. It was precisely as though a bucketful of cold water were to be suddenly flashed into the furnace of a locomotive engine, with the effect of extinguishing the fire, and bringing all movement to a standstill. Dr Currie has collected in his "Reports," many instances of the fatal effects of drinking copiously of cold water when the vital force is reduced by fatigue. Among the number, he quotes particularly Quintus Curtius' account of the catastrophe

that befell the army of Alexander on the banks of the river Oxus, when the loss of life was actually greater than had been experienced in any single battle with the enemy. The circumstances of this disaster were briefly these: a forced march of some forty-six miles in very hot weather over a desert plain, bodily exhaustion with excessive thirst, and the imbibition of largo draughts of cold water.*

The importance of the state of the skin in numerous diseases is well known to every practitioner of medicine. What surgeon does not feel confident that his patient is going on favourably, aftor even the severest operation, if his skin be but found soft and velvety? and it is the same in almost all diseases. Some acute diseases, I am inclined to believe, even prove rapidly fatal by inducing total, or all but total, suppression of the cutaneous exhalation. Among the number of these I cite Scarlatina in especial. I am confident that I have seen a patient die from this cause alone. Scarlatina, as is known to every one, is either a very trifling malady, or one of the most formidable and fatal with which, in the absence of other epidemics, we are called upon to deal. Speaking in the most general terms, two serious forms of the disease may be distinguished; one in which the throat is the part most seriously implicated, and in which, from an early period, typhoid symptoms take the place of the first more frankly inflammatory type of the disease. In this form, Scarlatina tends to destroy life less rapidly; it may run on for a fortnight, three weeks, or more, and prove destructive in the end by marasmus and exhaustion, usually associated with organic changes in the structure of the kidneys and lymphatic glands; or the constitution of the patient proving adequate to the heavy demands made upon it, he may make a slow recovery. In the other form of Scarlatina, the disease presents itself with the most frank inflammatory features from the first: smart

* See his Medical Reports on the effects of Water, Cold or Warm, on Fever and other diseases, 2 vols., London.

pain of the throat, great difficulty in swallowing, excessive heat of skin, and a pulse so corded and quick that it can scarcely be counted. In this form of the disease, even before it attains its status, the patient is sleepy, and by-and-by more drowsy or even lethargic ; the skin at this time is of the most vivid crimson, burning hot to the touch, marking 6° or 8° of Fahrenheit more than the standard, and seemingly perfectly unperspiring. This state, however, does not continue long; the drowsiness increases ; the patient can only be roused by shaking him, or speaking loudly into his ear, but he immediately relapses into his lethargic state when left to himself. The skin now declines from its first vividness of colour, the pulse loses in force, but increases so much in frequency that it cannot be accurately counted ; the patient is evidently in great peril, the utmost alarm is felt, and stimulants of all kinds—wine, brandy, ether, ammonia, are had recourse to with a view to sustain the visibly flagging powers of life, but commonly in vain ; for the patient sinks lower and lower, the efflorescence assumes a darker hue, the lethargic state passes into coma, the patient can no longer be roused, he has a slight convulsive fit, and expires. On making post-mortem examinations of the bodies of those who have fallen victims to this, the most acute or inflammatory form of Scarlatina, little or nothing is found amiss : the efflorescence has faded from the surface ; there is some suffusion and slight thickening of the mucous membrane of the tonsils and fauces ; some congestion of the brain and a little fluid in its ventricles—such is the unsatisfactory catalogue of necroscopic appearances, among which nothing of organic change is discovered to account for the fatal result.

This form of Scarlatina is not that which is most frequently encountered ; but in all epidemic visitations of the pestilence it has its victims. In a form somewhat less severe in its first stage the disease is extremely common, and when seen by the physician at this period it is of the last moment to the patient that he be treated properly, not by stimulants, as is too much the fashion at the present time, with a view to keep up the

strength, and so enable the system successfully to battle with the disease, but by tho sedative effects of cold and moisture to the surface, and such depressing medicines as tartar emetio and ipecacuanha, administered with a view to restore the function of the skin. It was whilst engaged in an extensive practice among the poor of London, as physician to the Infirmary for Children, now many years ago, the views of the vital nature of the function of the skin here advocated having already dawned upon me, that I was led to refer the fatal tendency of acute Scarlatina in its earlier stages to suppression of the cutaneous exhalation. Instead of resorting to cordials in order to support the powers of life under the state of apparent depression and drowsiness that then so commonly prevails, I bent all my efforts to restore the action of the sudoriparous system, the curative indication which here presents itself to me as being as indispensable, as it is necessary in cases of asphyxia from cold to supply the vital stimulus of heat, and in cases of asphyxia from drowning to convey warmth at onco from without, and by forced respiration to strivo to procure it from within. With the first indications of returning cutaneous transpiration, an immediate improvement in the patient's state never failed to be observed. The old-fashioned practice of exhibiting an emetic of ipecacuanha and tartrato of antimony, to be followed in an hour or two by a full doso of calomel, is greatly to be commended in such cases, and of itself and alone often suffices to put matters into a favourablo train; when tepid sponging of the surfaco so long as the skin continues hot and dry, with the aid of simplo saline and diaphoretic medicine, generally suffice to bring the patient to tho verge of convalescence. A little bark and cream of tartar will then put him beyond the reach not only of present disaster, but of any ulterior ill effects from his disease.

There is another means, however, of rendering acute Scarlatina manageable, which is far too much neglected in the present day; this is the affusion of cold or tepid water as first recommended and practised by Dr Currie, and that has only fallen

into discredit and disuse with the regular practitioner from the principles of its application not having been understood. When the heat of the skin is steadily above the standard, and the patient is inclined to drowsiness, the affusion of a pailful or two of water at 60° or 65° mostly acts like a charm ; if the disease is not cut short by the procedure, as seems often to be the case, it is usually rendered perfectly manageable in its subsequent stages. I admit that there is something outwardly formidable in such a measure as taking a patient in a burning fever out of his bed, setting him in a tub and pouring a pailful or two of cold water over his person : and it is this feeling doubtless that has had some share in causing Dr Currie's cold affusion to fall into discredit. But there is happily a modification of the process as free from everything apparently outrageous in its employment as it is efficacious in its effects. This is the process of *packing* with a wet sheet as practised by hydropathists. The patient is removed from his bed, stripped and enveloped from head to foot in a sheet wrung out of *hot* water, and being then wrapped in a thick, dry, and warm blanket, he is returned to his bed, and kept there for some fifteen or twenty minutes. He is then released, rubbed dry, clothed in a warm calico night-dress, and covered with a blanket or blankets, according to the season. If the practice have been efficient, as it mostly proves, the pulse will be found reduced both in force and frequency, the breathing freer and relatively rarer, and the skin, if not actually perspiring, yet soft, and no longer burning to the touch. The sleep that now so commonly ensues is natural and refreshing—in a word, the patient is found to have suddenly entered on a state of convalescence. The medical treatment already briefly epitomized will then suffice to complete the cure. Should a first application of the wet sheet and packing not have accomplished the desirable amount of curative effect, the process is to be repeated in some hours—fewer or more according to the urgency of the symptoms.

Formidable as the more acute forms of Scarlatina undoubt-

edly are, and in the suddenness with which their destructive tendency is manifested,—a suddenness that calls for the most energetic and well-directed remedial measures,—they are scarcely so much to be dreaded as those less acute forms of the disease in which the throat is particularly implicated, and the symptoms at an early period assume a typhoid character; when, after a brief period of manifest excitement, there supervene restlessness, delirium, a fluttering pulse and prostration, with fetor of the breath, sordes about the mouth, &c., &c. I am intimately persuaded that this form of the disease is due in no inconsiderable measure to its not having been seen in its first stage, or from its not having been dealt with resolutely on the principle epitomized above—a principle wherein *restoration of the function of the skin, suppression of which is the immediate cause of the condition that threatens life*, is the end to be kept in view. This is the form of Scarlatina that is so terribly destructive, and against which the resources of the healing art have still been found of so little avail. This is the form of the disease that sometimes sweeps off three, four, and even five members of the same family in the course of a few days, and perchance leaves the house childless that so shortly before seemed secure in its prospects of heirship and succession.

My purpose here is not to write a lengthened essay on the treatment of Scarlatina; I therefore but hint at the attention indispensable to the state of the throat—the use of a strong solution of nitrate of silver to the fauces, of tincture of iodine externally, and the application of a permanent water bandage about the throat—a measure most beneficial—of tepid sponging of the surface under the bed-clothes, the employment of simple saline and diaphoretic medicines, and the restriction of the diet to broths, milk, and farinaceous articles,—such stimulants as wine and ardent spirits not only doing no good, but being, as I believe, positively injurious. Children, the usual subjects of Scarlatina, are in my opinion under no circumstances the better for any product of the vinous ferment-

ation stronger than small beer,—and that they rarely like, and need not be compelled to take.

Thus far I have had under consideration that large class of general disturbances of the health known as Remittent and Intermittent Fevers, the consequences of climatic and atmospheric influences, and the single particular derangement of the system, which results from the absorption of a specific poison known by the name of Scarlatina. But fever is a generic title of very wide and very dissimilar import; and the diseases characterized as typhus and typhoid fever,—diseases that prove so destructive in the crowded quarters of populous towns especially, though their ravages are by no means restricted entirely to these,—are of a totally dissimilar nature from such as arise under the influence of exposure to the heats and chills of unhealthy climates, or the absorption of the poison that induces Scarlatina. It were foreign to my purpose to enter on the discussion of these important diseases in this place ; suffice it to say, that I am not so wedded to my own views of the nature of the sudoriparous function as to imagine that its implication is necessarily connected in the first instance with their production. I have for many years had too little opportunity of studying either typhus or typhoid fever, to give me any warranty for speaking authoritatively on the subject. These diseases are extremely rare in the district in which I have long practised—in the course of more than twenty years, I cannot remember having met with a greater number than five or six instances of continued fever that could be characterized as typhus or typhoid, although I have on various occasions been called to consult in cases of pneumonia that were believed to be fever of the kind. The instances in which I have met with simple continued fever I have always been able to connect with the foul effluvia and corrupted atmosphere engendered by mephitic drains and overflowing privies. I do not know, then, to what extent the function of the skin is implicated in the first and earlier

periods of continued fevers; that it is implicated in their course and even participates largely in the general derangement characteristic of these diseases, is made manifest by the irregularity and partiality with which it comes into play, and by the peculiar smell of the perspiration; more than this, copious sweating, instead of proving critical and preluding a happy issue in continued fevers, as it does in the paroxysms of an intermittent, is often but a sign of a disastrous termination to the case. In the present state of uncertainty, as regards the origin of continued fevers, and the interesting subject which these diseases present in their endless variety as studies for the pathologist, I naturally conceive that a careful investigation of the sudoriparous function in all their stages might yield a harvest of results that would be found of equal pathological interest and practical value.

I have room in this place only further to hint at the key which a knowledge of the sudoriparous function puts into our hands in interpreting the unquestionably beneficial effects that result from topical applications—fomentations, poultices, embrocations, blisters, solutions of the nitrate of silver, tincture of iodine and of the muriate of iron, &c., &c.,—in such a variety of local diseases. These applications impressing the sweat glands immediately, modify in various ways, or even upon occasion arrest their function over limited areas, and so influence the disordered actions going on in the parts beneath, —now stimulating them into greater activity, and so relieving torpor; and again repressing their action, and so abating such excitement as threatens organic change in the deeper seated tissues affected.

I began these essays by citing briefly, and in an English translation, the high authority of Berzelius, in regard to the prevailing ignorance of the end accomplished in the animal economy by the cutaneous exhalation, and I think I cannot do more to impress my reader with the importance of the sub-

ject that has engaged us thus far than by giving the passage at length in the excellent French version of the distinguished chemist's great work by M. Esslinger.* Here it is:—"Le but de la transpiration cutanée n'est point encore bien connu. La quantité des matières solides qui s'échappent du corps par cette voie se réduit a fort peu de chose, et ces matières se trouvent en outre dans l'urine, de manière qu' on ne peut pas considerer leur elimination comme en étant l'objet principal. *C'est surtout de l'eau que la transpiration cutanée entraine hors de l'économie;* et nous avons vu en traitant de la chaleur animale que la transpiration sert de régulateur pour l'abaissement de la temperature du corps, lorsque cette dernière a été portée a un trop haut degré par une exercise violent ou par la chaleur élevé de l'air ambiant. Mais la liaison intime qui existe entre la transpiration et l'etat de santé prouve qu'elle a été instituée dans un autre but encore, qui nous est inconnu. Elle peut sans que la santé en souffre, être diminuée par un refroidissement qui s'opère d' une manière graduelle ; mais lorsqu' une variation brusque de température vient de l' interrompre tout à coup, il resulte de la ce que nous appellons un refroidissement, source de la plus grande partie de nos maladies, et ordinairement suivi de ce changement dans les opérations vitales nommé fièvre qui frequemment se dissipe avec promptitude, mais qui peut aussi, même chez les sujets jeunes et robustes, entrainer la mort dans un laps de temps fort court. *Une explication exacte de ces phénomènes serait de la plus haute importance pour la médicine.*"

* Berzelius, Chimie, par Esslinger. Tom. vij. p. 330.

APPENDIX.

I AM admonished by a friend that I omit any mention of carbonic acid as one of the substances eliminated by the skin. The evidence on this head is, however, of so contradictory a character as almost to warrant the omission. Thus Messrs Cruikshank * and Abernethy † detect carbonic acid in considerable quantity in volumes of air in which a hand or a foot has been kept for some time; but Dr Priestley, ‡ on repeating their experiments, could discover none. M. Jurine, § again, concluded that carbonic acid was generated in the air in contact with the skin; this Dr Priestley || pointedly denies: he was never able to detect a trace of carbonic acid in air so circumstanced. And Drs Klapp and Gordon, ¶ with every precaution taken to secure accuracy of result, confirm Dr Priestley's conclusion. Mr Ellis, ** however, from certain experiments of Dr Mackenzie's, reports an evident production of carbonic acid.

These conflicting reports by able and conscientious men, suffice to assure us that the vital action of the sudoriparous system is in no way connected with the elimination of carbonic acid from the body. The deleterious nature of this gas when breathed has doubtless predisposed physiologists to disregard the fact of its perfect innocency when dissolved in the fluids of the body generally, and disposed them to look for some means besides the lungs for its elimination. But carbonic acid is a grateful tonic to the stomach, and is present in notable quantity without detriment in both arterial and venous blood, and in the urine, bile, &c. I imagine that the respectable authorities I have quoted were severally warranted in their conclusions. The sweat, as a direct educt from the blood, may very well hold a little carbonic acid in solution: when the skin is acting freely it is evolved in such quantity as to be readily discovered; when the perspiration is insensible, it is either absent or produced in so small a quantity as to escape detection.

* On Insensible Perspiration. † Physiological Essays. ‡ On Air.
§ Mem de Soc. de Med. || On Air. ¶ Ellis' Inquiry. ** Ib.

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